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TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

ELECTRICAL ENGINEERS.

SPECIAL MEETING, SEPTEMBER 16TH, 1890.

SUBJECTS:

Preliminary Report of the Standard Wiring Table Committee.

Resolutions on Adoption of American Names for Electrical Units.

SPECIAL MEETING, OCTOBER 21ST, 1890.

Investigation of the Stanley Alternate Current Arc Dynamo.

BY W. B. TOBEY and G. H. WALBRIDGE.

"A New Method of Analyzing Armature Reactions, Applied to the Stanley Arc Light Alternating Current Machine."

BY THORBURN REID.

PUBLISHED MONTHLY BY THE INSTITUTE, UNDER THE SUPERVISION OF THE EDITING COMMITTEE

R. W. POPE, *Secretary*,

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Vol
188

THE STANDARD WIRING TABLE.

DISCUSSION OF THE PRELIMINARY REPORT OF THE COMMITTEE BY THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, September 16th, 1890.

THE PRESIDENT (Prof. W. A. Anthony):—The first matter before us this evening is the report of the Standard Wiring Table Committee. The Secretary will read the action of the Council by which this was referred to the general meeting.

THE SECRETARY:—The Standard Wiring Table Committee was appointed by the Council some months ago, and has held several meetings, and at the meeting of the Council, June 17th, a preliminary report was submitted as appears from the following note in the minutes:

The preliminary report of the Standard Wiring Table Committee on Matthiessen's Standard, was presented by the Chairman of the Committee, Mr. Crocker. On motion of Mr. Wetzler, the report was received and ordered printed in the Transactions. On motion of Mr. Maver, it was resolved to submit this preliminary report to a general meeting of the Institute for action."

MR. GEO. M. PHELPS:—Will you please read the preliminary report?

Mr. Crocker read the preliminary report as follows:

PRELIMINARY REPORT OF THE "STANDARD WIRING TABLE COMMITTEE."

In Regard to Matthiessen's Standard of Resistance of Copper.

The Council of this Institute at its regular monthly meeting held December 3, 1889, appointed a Committee "to formulate and submit for approval a standard wiring table for lighting and power purposes."

The Committee appointed consists of the following members of the Institute: Thomas P. Conant, Dr. Louis Duncan, Prof. Wm. E. Geyer, A. E. Kennelly, George B. Prescott, Jr., E. Wilbur Rice, Jr., Prof. E. P. Roberts, Prof. Harris J. Ryan, William Stanley, Jr., Dr. Schuyler S. Wheeler and Francis B. Crocker, Chairman.

This action was taken by the Council with the object of overcoming or reducing the great confusion which now exists in regard to the standards and constants of electric conductors.

At the first meeting of the Committee held January 10, 1890, it was decided to confine the work at first to the three subjects of Standards of Resistance; Temperature Co efficient; and Safe Carrying Capacity of Copper, since these are of fundamental importance.

The subject of Matthiessen's Standard alone is so confused and involved, and the discrepancies are so great between the best authorities that the Committee has devoted its attention almost entirely to this subject up to the present time.

After a very thorough investigation of Matthiessen's work with a view to ascertaining what his Standard really was, by a sub-committee, consisting of Prof. Wm. E. Geyer, Mr. George B. Prescott, Jr., and the Chairman, the conclusion has been reached that Matthiessen's "mile standard" (one statute mile of pure copper wire 1-16 inch in diameter has resistance of 13.59 *B. A.* units at 15.5° *C.*) is not the true one although very com-

monly used. We consider the correct Matthiessen's Standard and the one in which he himself had most confidence was the "metre-gramme" standard (resistance of pure hard drawn copper wire, one metre long weighing one gramme — .1469 *B. A.* Units at 0° *C.*) This has the disadvantage that it is expressed in terms of weight instead of diameter. It is, however, very much more difficult to get the exact mean diameter of a wire than to get its exact weight as Matthiessen himself states. Furthermore, by selecting the most reliable constants given by Matthiessen and corroborated by investigation and correspondence with the best authorities, we have by calculation converted this "metre-gramme" standard into a standard referred to dimensions and independent of weight, which is the form generally used.

The various constants selected, the method of calculation and the values deduced are given in a table at the end of this report.

As to the fact that wires may be found which test 102 per cent. of Matthiessen's standard or even higher, we are of the opinion that this is no real objection provided the value of the standard is definite and generally accepted. A standard which is not the highest attainable value may even be considered an advantage since the average commercial wires will approximate to it more closely.

Although we believe the standard we recommend will answer the purpose temporarily and probably permanently, nevertheless, we think that if a thoroughly correct and complete redetermination of the standard resistance of copper could be accomplished, it would be a benefit to electrical science and industry. Favorable offers in this direction have already been received by this Committee from Johns Hopkins University, Cornell University and Columbia College, and it is very likely that this redetermination may be undertaken.

This Committee purposes next to take up the subject of the safe carrying capacity of wires.

Matthiessen's standard recommended by this Committee is:

A hard drawn copper wire 1 metre long weighing 1 gramme ("metre-gramme") having resistance of .1469 *B. A.* units at temperature 0° Centigrade.*

From this standard we calculate by taking the value 8.89 for the specific gravity of copper, that a hard-drawn wire 1 metre long and 1 millimetre in diameter ("metre-millimetre") has a

* *Philosophical Magazine*, May, 1865.

resistance of .02104 *B. A.* units at 0° *C.* This value is also given by Matthiessen.†

Matthiessen's figures ‡ for relative conducting power are:

Silver	100
Hard or unannealed copper	99.95
Soft or annealed copper	102.21

From these the resistance of hard copper is found to be 1.0226 times that of soft copper, therefore the resistance of a soft copper wire 1 metre long and 1 millimetre diameter is .02057 *B. A.* units at 0° *C.*

From this the resistance of 1 cubic centimetre of soft copper is found to be .000001616 *B. A.* units at 0° *C.*

And the resistance of soft copper wire 1 foot long and .001 inch in diameter (mil-foot) is 9.720 *B. A.* units at 0° *C.*

Taking one *B. A.* unit as .9889 legal ohm any of the above values may be converted into legal ohms. To find the conductivity of copper at temperatures other than 0° *C.* Matthiessen's formula may be used $C_t = C_0 (1 - .00387\, t + .000009009\, t^2.)$

TABLE OF VALUES BASED UPON MATTHIESSEN'S CORRECT STANDARD.

	B. A. Units. 0° C.	Legal Ohms. 0° C.
Matthiessen's Standard metre-gramme, hard	.1469	.1453
Metre-gramme, soft	.1436	.1420
Metre-millimetre, hard	.02104	.02080
Metre-millimetre, soft	.02057	.02034
Cubic centimetre, hard	.000001652	.000001634
Cubic centimetre, soft	.0000001616	.000001598
Mil-foot, hard	9.940	9.829
Mil-foot, soft	9.720	9.612

Specific resistance of hard copper (1 cub. cent.) = 1634 (C. G. S. units.)
Specific resistance of soft copper (1 cub. cent.) = 1598 (C. G. S. units.)
Matthiessen's Standard specific gravity of hard copper, 8.89.
Resistance of hard copper is 1.0226 times that of soft copper.
Resistance of soft copper is .9779 times that of hard copper.
Legal ohm is equal to 1.0112 B. A. units.
B. A. unit is equal to .9889 Legal ohms.

F. B. CROCKER,
Chairman.

(NOTE.—This report has since been revised in accordance with the instructions of the meeting of Sept. 16, 1890. R. W. POPE, Secretary.)

† *Philosophical Magazine*, May, 1865.
‡ " *Transactions*, 1864.

Mr. Crocker continued as follows:

"Of course the derived values follow as soon as the fundamental standard is decided on, and the question therefore is, not what the derivations are, but whether the fundamental standard is correct; in other words, whether Matthiessen's 'mile standard' or Matthiessen's 'metre-gramme' standard is the more reliable. Now the sub-committee has investigated this particular point with a great deal of care and arrived at a very definite conclusion: that the metre-gramme standard was unquestionably the more accurate and the more reliable of the two, and that Matthiessen himself so considered it. Fortunately, after having decided this, we received a letter from Mr. Latimer Clark, who is one of the best authorities on this matter, and who was a contemporary of Dr. Matthiessen: in fact he suggested to Dr. Matthiessen that he undertake these experiments, so he is especially competent to express an opinion on this subject, and he agreed with our conclusions in every respect. We had already arrived at these conclusions, so his letter was simply corroborative. Mr. Latimer Clark's letters to the Committee covered the subject very thoroughly and clearly, and I wish here to express our thanks to him for his kindness. There is, however, considerable confidence in the mile standard, and I think simply for the reason that it has been used, and the fact that a mile, and the sixteenth of an inch are more familiar values for the ordinary American and English engineer than a metre and a gramme, but the metre-gramme is just as capable of being transformed into practicable and common units as the mile, and sixteenth of an inch. The necessity for deciding on some standard is more urgent than would be ordinarily supposed. This morning for instance, in my ordinary professional work, I referred to two wiring tables issued by presumably competent persons, and both intended to be correct and both being such as an engineer would use in his practice. One of these tables gave the resistance of No. 25 B. & S. gauge copper wire as 33.4 ohms per pound at 75 deg. Fahrenheit. The other gave it as 34.7 ohms per pound at the same temperature. Now there is a difference of 1.3 ohms in 33 ohms, which is about 4 per cent. In some cases it does not make very much difference whether it is 4 per cent. one way or the other, but 4 per cent. is a very considerable discrepancy, and in a great many cases it would make a very serious difference. For instance, I was figuring on sixteen pounds of wire and it made a difference of almost 21 ohms. That is not only measurable, but it is a serious matter in some cases. I think that when two wiring tables that electrical engineers ordinarily use, differ by 4 per cent., it is time to do something. I just happened to pick up these tables this morning. I did not do so with reference to this subject. I dare say if I looked for discrepancies I could find much greater ones. The adoption of this standard does not mean that every wire used for commercial purposes shall be 100 per cent. of Matthiessen's stan-

dard. There is no reason why an electrical engineer in his specifications should not require 101 per cent., if he considers it necessary, and I can think of cases where it might be necessary. On the other hand, it might be sufficient if the conductivity was 97 or 98 per cent. of the standard. It is simply necessary that we should have some definite value.

The question whether any standard at all is necessary is a more important one; as it is not absolutely necessary to have a standard. We could simply say that one mil-foot or one cubic centimetre of the copper used for a certain purpose should have such and such resistance—in other words a certain specific resistance. But practical men have become accustomed to using Matthiessen's standard, and they seem to want a standard, and they will use one whether we want them to or not; such being the case I think it better to decide what Matthessen's standard is. It certainly is commonly used. You see the expression in every book and almost every paper and specification, and as it now has no definite value—even though it is not used in the future, just simply to have it settled once and for all, I think it would be well to settle upon a definite standard. We feel sure that the Institute will make no mistake in adopting this as Matthiessen's standard, and unless there is some particular objection to it which has not been brought out by our investigations, we hope the Institute will do so.

Mr. George B. Prescott, Jr.:—In making this report, the Committee has not thought it necessary to go into details very much, and it may be interesting to members of the Institute to know how this discrepancy in what is called Matthiessen's standard has arisen. In the first place, it is perhaps worth while to inquire why this standard was adopted in preference to any other. The reason was, because Dr. Matthiessen made a series of careful experiments to determine the resistance of metals and the variation of their resistance with changes in temperature, which extended over a great number of years and formed the basis of the work of the British Association which decided on the practical standards. It seems that years before this British Association Committee undertook this labor, various standards of resistance had been proposed by German, French and English electricians, and they were of all kinds, and among them was one proposed by Dr. Matthiessen, which was this "standard mile." There were no ohms in those days, and he made up a standard mile of this copper and that was used as a unit in England for a long time. Later on, when the Committee of the British Association had determined the value of the ohm in absolute units and had prepared practical standards, part of this Committee tested the different standards that had been made in different parts of the world, including this standard of Matthiessen, and when they measured it they found that it was equal to 13.59 British Association ohms. Matthiessen never gave it that value; it was sim-

ply an arbitrary standard that he selected, because it was a convenient unit. Probably it was the size of wire commonly used in those days. So that really it never was a standard except in that sense. After Matthiessen's work was completed and published, Dr. Fleeming Jenkin, who was the Secretary of the Association, tabulated the results. Matthiessen's measurements were made with respect to weight and length, and also diameter and length. But his measurements of diameter were always very unsatisfactory, and finally the only measurements that he really relied on were those of length and weight. But he made a very great mistake, which he himself acknowledges in his reports, which was, that he neglected to take the specific gravity of the actual samples that he experimented on, and the result, of course, is easily understood. Every sample of copper varies somewhat in its physical properties, and if you were to take one hundred samples of wire and measure their resistance at a given weight and length, and then attempt to calculate the resistance from the cubical dimensions, you would find a difference, simply because there is a difference in the drawing of wires which changes their molecular constitution. Dr. Matthiessen neglected to take the specific gravity of any sample of wire that he tested, and the result is, that we do not know to-day what Matthiessen's standard is except in terms of weight and length.

A further complication arose in this way, that Dr. Fleeming Jenkin, who as I say, tabulated the results which Dr. Matthiessen obtained; made all sorts of derivations, giving the resistances in foot-grains and foot-mils and metre-grammes and metre-millimetres—all by calculation. Of course in converting from length and weight into length and diameter he had to use specific gravity. He selected certain specific gravities. In his original table there are errors. Those errors have been copied. Other people have calculated tables from the standard mil unit, and of course they give different results, so when Matthiessen's standard is referred to to-day, there is a great deal of uncertainty as to which standard is meant. As a matter of fact, the statute mile standard is probably nearer the true resistance than the metre-gramme, for the reason that it gives a higher conductivity, and there have been any number of cases of wire being tested that had greater than one hundred per cent. conductivity, and it was for this reason that seven or eight years ago I calculated a wire table, and selected this unit of Matthiessen's simply because it was the lowest, and agreed with some results that I had actually obtained.

Mr. George G. Grower:—I think there is a mistake made when hard copper wire is assumed as the standard and the resistance of soft copper reckoned from it. This takes for granted that there is a fixed ratio in resistance between hard and soft copper. But this ratio depends entirely upon the hardness of the hard copper compared with the soft. For instance, if you

take large copper wire and have it as hard as you can get it, the conductivity would be almost as good as soft copper, while in a small wire it would not be nearly so good. I have had as much as eight per cent. difference between hard and soft copper made out of exactly the same stock on small wires. If we should make the wire very small, I dare say it would make still more difference.

I agree with the Committee that the metre-gramme standard is the more reliable, because whenever I have tried to make tests with diameter I could not get any sort of agreement among them; but where I take soft copper standard, no matter what the size of the copper is or anything else, so long as it is soft copper, and from the same stock, I get almost the same result. But if you take hard copper for the tests, the results will differ several per cent., according to the size of the copper. I think for that reason the standard should be soft copper of a given length and weight.

Mr. Crocker:—It is perfectly correct as an abstract statement that soft copper is more constant in its resistance than hard copper. Hard copper can be made of almost any resistance up to several per cent. above soft copper. But soft copper is not Matthiessen's standard. It is precisely like the case of the foot. The English foot originally, I have heard, was the length of the King's foot. Well, now, at the present day, I think it is a little in excess of the average length of the human foot. But it does not make a particle of difference, provided it has a definite value, and it has, and providing a correct copy of the standard is kept in a safe place—it does not make any difference whether it is an inch longer or an inch shorter than the human foot. It has a definite value, and no one, I think, at the present time, would recommend changing the English foot, as a unit of measurement, to agree more closely with the length of the anatomical English foot. It is precisely the same with Matthiessen's standard. Unfortunately or fortunately, as you may look upon it, Matthiessen has promulgated a standard and it has been largely, in fact almost universally accepted. Now all that is necessary is to adopt a definite value for this standard. It does not make much difference what it is, provided it is definite and generally accepted. Of course in selecting a definite value we want to select the best one, the most reliable one, the one that he has himself considered most reliable, and the one that other authorities deem the best. So it seems to me that the mere fact that one standard or another approximates more closely to what the highest possible value is, or the fact that one or the other happens to be easier to determine in practice, makes no great difference, provided we get a certain definite standard or resistance for various lengths, weights, diameters and kinds of copper. I think that all that is necessary is to free this subject from its great confusion by simply adopting a definite standard, and if that stand-

ard happens to be more correct than some other, so much the better; but even if it is not, it is very much better than confusion.

DR. LOUIS BELL:—As regards this question of standards, I suppose that standards must be established, but it is not a pleasant task for the man who has to do it. Of course everybody now understands perfectly well that Matthiessen's standard does not represent the actual resistance of chemically pure copper, either soft or hard. It represents those particular specimens of copper which he had in hand, and fortunately happens to represent something that is pretty nearly good commercial copper in every day use. Now what is to be gained in going to work and obtaining the purest copper that the chemist's art can give us and then working out all our wiring tables on that basis, if after all this labor and trouble, we have the result that we are compelled every time we calculate commercial copper to multiply the values of those wire tables by .984362 or some such unpleasant decimal, which would make everybody very tired who had the misfortune to have anything to do with it. If we had a perfectly sharp and definite standard of the purest copper obtainable, we would not be able to use it, but would have to transform it to some other value which meets commercial conditions. I very cordially agree with the recommendation of the Committee to take as a standard something that is generally accepted and does represent what we can reach in commercial practice. With a table made up on that basis a man can buy his wire with a certain feeling of confidence that its resistance is going to be pretty nearly what is laid down in the table, and that he will not have to multiply by any complex fraction to find what the real resistance of the sample he has bought is likely to be. The Matthiessen standard recommended by the Committee, simply does represent the commercial copper of the day; not very good commercial copper either; and for this reason that it does represent what we can readily obtain and what we can count on buying, I believe this standard even though it is not the strictly absolute standard of copper resistance, is an excellent practical working value to use until we have some complete determination of the conductivities of metals. There is, by the way, great need of that, and I hope it will be done. I hope the offer received by the Committee will be taken up and that the subject will be worked over, because in the resistance of metals other than copper, there is a still greater discrepancy than we find in copper. But pending that, and perhaps for future time quite independent of the results that may be reached in such a research, I believe that the practical standard proposed by the committee is an exceedingly good one.

MR. CROCKER:—In regard to what Dr. Bell said about the Matthiessen standard, fortunately agreeing with ordinary commercial copper, I think that is a strong recommendation in its favor. I see no practical reason, certainly, for spending months in ob-

taining copper purer than has ever been obtained before, and purer than any ordinary user of copper can ever expect to obtain, and determine its resistance, and then call that the standard. There may be some scientific reason for it, but there is certainly no practical reason. Now a standard like Matthiessen's is simply a commercial standard. It is used incidentally in scientific work as a definite value, but that is not its primary object. Its primary object is an ordinary, definite, practical standard of resistance of copper, and the nearer that standard approximates to commercial averages, so much the better; and if we succeed in getting a wire which is 103 per cent. of Matthiessen's standard, and I think it would be possible to do that, I do not see that it would vitiate the standard a particle. I think Dr. Bell rather overrates the average conductivity of commercial wire. I think if one succeeds in getting wire of 100 per cent. Matthiessen's standard every time, he would be doing very well, and I think that most specifications do not call for more than 96 to 98 per cent. Matthiessen's standard, which shows that the standard is such that the very best commercial wire just about reaches it and ordinary wire just falls below. The highest point at which reasonably good copper wire would go under commercial conditions, I think is exactly where the standard ought to be. I suppose the tendency is for the conductivity of copper to go up all the time with improved methods of manufacture, but I doubt whether it ever gets much above Matthiessen's standard, and I think it may get so that the best copper is about equal to Matthiessen's standard. So, as Dr. Bell says, it fortunately agrees with commercial values.

Mr. William Wallace:—There is one thing to be considered, —that there are so many degrees of hardness and softness. You can get copper extremely hard or soft and get all the range between, and that plays a very important part in this question. I have heard very little said about that. It is a thing, I think, that ought to be looked into.

Mr. William B. Halsey:—How would it do in practice if silver were used as the metal which the standard should be made of? Is there as much variation in the hardness and softness of silver as there is in copper? Or would aluminum be a better metal yet?

Mr. Prescott:—I can answer that. Dr. Matthiessen used silver as the standard, and a great many of the discrepancies that occur in his results are due to the fact that at different times he used different standard wire, so that when he came to compare a copper wire that he had tested with one silver standard and then later by another silver standard, he would get discrepancies, and all through his report you will find difficulties of that sort which he himself pointed out. There is one other point that I would like to mention—that is the question of the temperature-coefficient for copper. Mr. Crocker, I think, neglected to read the value

given by Matthiessen, which we have picked out very carefully; and I want to call your attention to another curious error that has crept into the text-books in regard to this temperature-coefficient. Matthiessen gave the value in terms of conductivity at zero temperature multiplied by a certain constant. This constant is composed of three terms, and there is a minus sign before the second one. Now conductivity is not so commonly used as resistance, therefore to make this formula generally useful the text-books have converted it into terms of resistance and in doing this they have invariably, so far as I have found merely changed the signs. What they should have done was to take the reciprocal of this value and the reciprocal changes second term of this constant, and that correction has never appeared anywhere that I am aware of excepting in an article which I published in the *Electrical Engineer* some little time ago calling attention to it. This is rather an interesting thing. Matthiessen's formula for the temperature coefficient is this:

$$Ct - C_0 (1 - .00387t + .000009009t^2$$

This same formula is also commonly used for resistance, merely with the first sign changed. But if the correct reciprocal value is taken, the value of the second term will be considerably altered.

Dr. William B. Geyer:—I suppose that this report, so far as the verbal part of it is concerned, is more or less provisional. As a member of the sub-committee on Matthiessen's standard, I am perfectly familiar with all its contents. But a few days ago, I wrote a letter to the Chairman, suggesting certain changes in the sense expressed by Mr. Grower, namely: that we ought not to speak of Matthiessen's standard as being the resistance of a certain sample of hard copper, but we ought to speak of it as being the resistance of a certain sample of soft copper, which latter Dr. Matthiessen also gives, and the reason is, as Mr. Grower just said, that hitherto all coppers, even hard copper, have been compared with pure annealed copper as being the highest conductivity generally known, and if that report is provisional, as far as the wording goes; I would again suggest that it be modified in that direction.

Mr. Crocker:—I think that objection can be overcome by simply stating that Matthiessen's standard for hard copper is such a value, and for soft copper is such another value, giving them equal prominence, they being unquestionably Matthiessen's standards for hard and soft copper respectively. In other words there is no reason for pushing one of them ahead of the other. They are derived from each other directly, and this objection seems to be really more verbal than material.

Dr Geyer:—I think there is an objection to speaking of both hard and soft, especially as hard coppers are of all degrees of hardness. It is true, too, that soft coppers are of all degrees of softness; but when we speak of annealed copper we think of it

as being annealed as well as it can be, and practically all coppers, hard and soft, should be compared with something that is supposed to be perfect.

MR. GROWER:—In regard to the relative degree of hardness and softness of copper, I might say that out of numerous tests I have made with copper, half an inch or over in diameter down to one or two thousandths only, I have not found as much as a half per cent. difference in soft copper. Although I never tested any very small hard copper, I got as much as eight per cent. difference, nearly nine per cent. difference, between different standards of hard copper or different sizes made of the same stock. Precisely on that ground alone I think that is is not right that any standard of hard copper should be accepted, because it leads to the inference of a certain ratio between the conductivity of hard and soft copper. I think with very small hard copper, we could get fifteen or twenty per cent. difference. The wire that I tried was not small. It was about No. 22; I am not sure precisely what it was.

THE PRESIDENT:—It would seem to me that it would be better to recommend only one standard, from the fact that what we are after is to establish some sort of a standard on which wiring tables can be computed. It is not the idea, as I have understood it, that we are trying to establish a standard material from which the resistance of copper could be determined. That is, if a person has lost all his standards of resistance he could not go and pick out a piece of copper wire and measure off a piece a metre long and weighing one gramme and say this shall have such and such resistance, and take that as a standard and go to work and determine the relative resistances of other pieces. That would be out of the question He could not obtain a sample of copper exactly like what Matthiessen used. But what we do want to do, is to fix upon some standard length and weight, or length and diameter as having a certain resistance. That is, we shall say that a certain piece of copper of any certain degree of hardness or purity, of a certain length, at a certain weight, has a certain resistance, and then go to work and compute our wiring tables, putting in all that we ordinarily put into wiring tables. Now we have something that is perfectly definite. This wiring table says that a copper wire a mile long and one-tenth of an inch in diameter, we will say, measures 5 ohms. Very well. If we find that we have a piece of copper wire that is a mile long, and one-tenth of an inch in diameter, that measures 4.9, why the resistance is 98-100ths. of the standard. We do not care what the standard is. So that it seems to me that what we want to adopt here is some one standard, not two. We do not want two wiring tables. We have half a dozen wiring tables now; and it is just what we are after to get rid of different values, and if this Institute adopted this standard to-night with these two values in it, it seems to me that we should find somebody by-and-by calculating a wiring table on one of

them and publishing it and sending it out all over, and somebody else would calculate a wiring table on the other standard and send it out also, and then we should have two wiring tables.

DR. GEYER:—Another reason suggests itself to me as to limiting the standard to one value, and more particularly the value of soft copper. There are two valuable qualities in copper sought after—conductivity and tensile strength. If you want to get the tensile strength you lose conductivity. If you want to get conductivity you lose tensile strength. Now, if the two things could be combined in one, that would be a very valuable property. Suppose one manufacturer aims at tensile strength and he is asked what is your conductivity. Well then, he will probably calculate by hard copper and say, "Oh, my conductivity is so high—very high indeed." Then this sample will be sent to somebody for testing. He will examine it and perhaps use a hard copper standard, or perhaps use a soft copper standard, or perhaps report on both, and in that way confusion is at once introduced. Let a man make up his mind what he wants—whether he wants conductivity and sacrifices tensile strength to that, or wants tensile strength and sacrifices conductivity to that.

MR. CROCKER:—I agree with the President and Dr. Geyer in the inadvisability of having two standards. I forgot for the moment that two would cause confusion. I think that one standard had better be adopted. The avoidance of confusion is paramount.

THE PRESIDENT:—I think that the soft copper is the better standard for the reason that soft copper is much more definite than hard copper. The hard copper, as I understand it, varies so greatly in hardness that if we have a hard copper standard it is not likely that any other hard copper would agree with that standard. We might have a hard copper that differed by four per cent. from soft copper, or a hard copper that differed by one per cent. from soft copper; so that the hard copper standard would not have any definite relation to the hard copper standards in commercial use; while with annealed coppers of the same purity we may expect that they will pretty well agree with each other.

MR. FRANKLIN L. POPE:—I move that the provisional report of the Committee be adopted with the recommendation that the standard be expressed in terms of soft copper—Matthiessen's value.

The motion was seconded and carried.

THE PRESIDENT:—Now it seems, as this is only a preliminary report, the matter is still in the hands of the Committee for further action by-and-by.

MR CROCKER:—I should rather have the matter definitely decided to-night. I doubt if it will be again taken up by the Institute for some time to come, and there seems to be no great conflict of opinion in regard to it.

MR. GEORGE M. PHELPS:—I understand that we have now definitely adopted the report of the Committee as amended by the suggestions that have been made and that the Matthiessen's metre-gramme standard is adopted. That seems to me to be disposed of. But I understand the President implied in what he said that the Committee perhaps had better be continued to finish the rest of its work, and therefore I move that the Committee be continued to report at some later convenient time.

MR. PRESCOTT:—On what?

MR. PHELPS:—You were going to get up some wiring tables.

MR. CROCKER:—There is lots of work ahead for the Committee and the Committee proposes to undertake it, but it would relieve our minds very much and encourage us to go farther and do better if we felt that part of our work had been accepted.

THE PRESIDENT:—The Chairman understood that this motion adopted the Matthiessen standard for soft copper as reported by this Committee as the expression of this Institute; that that part had been adopted and settled, and the suggestion was that the Committee should now proceed to the further work, as this is only a preliminary report.

MR. PRESCOTT:—As I understand it, the Committee will make this revision that has been suggested in this report and that then it will be published in the Transactions without further submission to the Institute.

THE PRESIDENT:—That was the understanding of the Chair.

MR. PHELPS:—I think that is quite important, Mr. Chairman, although it would not be exactly in conformity with what has taken place here; but it would be important that this report, when it is published, should be quite clear. Therefore, as Mr. Prescott suggests, that report before printing should be revised in the sense in which it has been accepted by the meeting. Then it would not be open to any one to make any difficulties on the record.

A MEMBER:—I would like to ask whether the gentleman referred to some action of Johns Hopkins and Columbia Colleges and some other institutions in reference to the subject, and if the Committee have any recommendations to make.

MR. CROCKER:—The Committee at first thought that a standard should be detemined with the best modern instruments and with the best attainable copper but what we require after all is simply a definite standard, and although it would satisfy our curiosity to have the highest attainable conductivity of copper detemined, I doubt if it would benefit science or electrical industry very much to have it done. At one time we had decided to go ahead in that direction and we were very much encouraged by the offers of assistance which were given to us. but I think now it would simply be a matter of deciding the highest attainable conductivity in terms of Matthiessen's standard, which of course would be an interesting thing to find out, and it might possibly

be used as a standard of some sort or be of benefit to electrical science in other ways.

MR. ROBERT S. DOBBIE:—If this metre-gramme standard is decided on, it is equally important that the specific gravity should be determined. Do I understand from the Chairman of the Committee that we had definitely decided on a certain value given to Dr. Matthiessen?

MR CROCKER:—Yes, it follows as a matter of course that a certain value of specific gravity is Matthiessen's value. But this is all arbitrary. Unfortunately Matthiessen did not determine the specific gravity of his particular sample. But the only specific gravity which will make his two values agree is 8.89, which therefore is Matthiessen's value for the specific gravity of copper.

MR. ROBERT S. DOBBIE:—That is the specific gravity of soft copper?

MR. CROCKER:—No, hard copper. We simply take the value given by Matthiessen for soft copper, and adopt that as our standard. That involves this specific gravity in deriving it, but after the standard is once arrived at and adopted, the specific gravity used in getting it ceases to be important.

MR. DOBBIE:—That soft standard has been adopted by the vote of the Institute.

MR. CROCKER:—Yes, and that necessarily assumes specific gravity in deducing it, but now it applies to all copper of all specific gravities.

DR GEYER:—The determination of the highest conductivity obtainable in copper is, I think, simply an interesting scientific question not having much practical importance. I think the determination of the specific gravity of commercial copper is of some practical importance, and I have proposed to myself, at an early opportunity, to get a large number of samples of commercial copper in order to see what really is the specific gravity of commercial copper. As Mr. Crocker has stated, the specific gravity, 8.89, is simply one deduced from Matthiessen's figures; it is to that extent an assumption. We are compelled to take that, in order to harmonize the conductivity in terms of weight and length, as stated by him, with the conductivity in terms of length and diameter as also stated by him. Possibly he made the determination of a sample and found that specific gravity, although, as far as I recollect, it is not definitely stated. Now, if it should be found that the specific gravity of commercial copper is 8.89, then it will be perfectly correct to go from his metre-gramme system where resistances are stated in terms of weight and length, to a system which is more commercial, where resistances are stated in terms of length and diameter.

A MEMBER:—I would like to ask—and this is a question which has attracted a good deal of attention across the water, especially in England and Germany—how this standard would compare

with the present knowledge of the matter in France, England and Germany.

Dr. Geyer:—As far as the standard goes, I think in Germany they very largely refer conductors to mercury, which has some scientific advantages, though practically it is not a good way to do it, and in this country I do not think it will be ever introduced where people have got into the habit of referring to the Matthiessen standard.

Dr. Bell:—It does not seem necessary to offer any more embarrassing suggestions in the way of the Committee; but there is one point mentioned by Mr. Crocker to which I would like to take exception, and that is the legal ohm part; because it happens that the British Association at its Leeds meeting has recommended a new value for the legal ohm which ought to come under careful consideration, recommending the standard mercury column 106.3 centimetres long instead of 106 as assumed by the Paris Conference in 1884, and that change will probably come up in active form very shortly. So that I simply throw that out as another little suggestion to the Committee in their already complicated task.

Mr. Crocker;—I think that the Committee would have to have a daily cablegram from London to keep themselves up to date on the question of the value of the legal ohm, and the Committee has no appropriation for cablegrams. Possibly it would be better to give Matthiessen's standard in terms of B. A. units, and then from time to time, as the whims of foreign electricians went one way or the other, we could modify our figures to suit the legal ohm at that particular time. In fact, Matthiessen's standard, bad as it is, is about as definite as the ohm.

Mr. Prescott:—Although we have adopted Matthiessen's value for soft copper definitely, and I think there are good practical reasons for doing it, as a member of the committee I would like to show the historical and logical reasons for adopting the hard standard which we did. The wires that Matthiessen tested were all hard samples, the ones that he made the ultimate measurements of. He determined the relative resistance of soft copper by comparing pieces of soft copper with the resistance of hard samples which he had measured, and he did not make, so far as I have been able to ascertain, any distinct measurements of soft copper, but derived it purely by relative measurements.

THE UNIT OF SELF-INDUCTION.

The Secretary:—The next subject to be discussed and acted on is the adoption of an American name or names for one or more practical electrical units. This matter was also referred to this meeting by the Council, June 17th. The resolutions have been printed in all the papers, and I have them here if the members wish them read.

THE PRESIDENT:—Do you desire the reading of the resolutions?

The reading of the resolutions was called for and the Secretary read them as follows:

Whereas.—It has been the custom in the nomenclature of electrical units to perpetuate the names of men who have contributed most to electrical science, and

Whereas.—In the names thus far adopted the eminent services of Americans have not been recognized, therefore

Resolved.—That in the opinion of the American Institute of Electrical Engineers, a just distribution of the honors thus bestowed necessitates a recognition of the splendid contributions to electrical science of one or both of America's great electricians—Benjamin Franklin and Joseph Henry; and,

Resolved.—That this Institute will gladly co-operate with other bodies in this country and abroad to secure the general adoption of these names for electrical units; and,

Resolved.—That the name of Henry should be given to the practical unit of self-induction, since he was the discoverer and greatest investigator of this phenomenon, and because this unit at present is called a quadrant, which is merely a numerical value and not a suitable name.

Resolved.—That this Institute recommend to electrical societies and electrical engineers the general use of the name Henry for the unit of induction as being the quickest and surest way to secure its final adoption.

DR. SCHUYLER S. WHEELER: Mr. President, is not a resolution now before the Institute?

THE PRESIDENT:—The resolution just read is now before us, having been referred to a general meeting by the vote of the Council.

THE SECRETARY:—I do not understand that it is to be finally acted upon at this meeting.

DR. WHEELER:—Is there any difficulty in acting upon it finally?

MR. CROCKER:—None that I am aware of, and I urge that it be acted upon unless there is some objection. I introduced this resolution at the Council meeting and hoped for some action there, forgetting that it was hardly within the province of the Council to take final action on a matter of this sort, and it was deemed best to refer it to a general meeting of this Institute, but I think that is sufficient and that a general meeting of the society is perfectly competent to act on this matter. As to the advisability and propriety of doing it, I see no objection. In fact the idea has been received very favorably by electricians abroad, and the only point that there can be any question about is whether Henry is clearly entitled to the discovery of self-induction. Henry's orginal paper, which antedates by two years anything that Faraday did in that direction (the only other man who could be entitled to be called the discoverer of this phenomenon) was printed in the year 1832 in Silliman's Journal and distinctly describes the phenomenon and states that it is due to the action of the current on itself, that is to say—self-induction.

As to the word "henry" for a unit, I think it is rather better than the average. Some of the names of electrical units are very good. They are almost all of them unique and different from

anything else. Ohm, Volt, Ampere, Coulomb and Henry never would be mistaken for anything else, because they are proper names and the context would always clearly distinguish them. Henry seems to be eminently suited to be the name of an electrical unit. It is shorter than ampere and coulomb. It is as short as joule and farad and is almost as short as the very short words ohm and volt. It only differs from volt which is an extremely short word, by one letter. So I see no reason for abbreviating it, although it has been suggested by Dr. Fleming to abbreviate it to "hen," which I think is open to several serious objection. The first is that the names loses its identity. Faraday may be abbreviated to farad and everbody would know what it came from, whereas if you abbreviated it to "far" nobody would know what it meant. And the same way with henry. I think if it is abbreviated to "hen" it loses its identity entirely.

Now I think it better not to state exactly what the value of a "henry" is. Whatever the unit of self-induction is decided to be, let that be called the "henry." If we fix a certain value to the "henry," and the unit of self-induction is made something else, that would cause trouble. So I think we had better leave a blank for the value of the "henry." The present unit, in my opinion, is rather high and it might be cut down.

The number of units that have been allotted to different countries is rather interesting. We have farad, watt, and joule for England. We have ampere and coulomb for France We have ohm for Germany and volt for Italy. Those are all I recollect now. I think that England and France had better wait until the other countries catch up to them. Dr. Fleming tries to put in another English name with this American name. In that way the English would get another unit every time any other country got one, and thus keep a controlling interest in units as it were. But I do not think that is at all necessary. They already have three, and although their contributions have been large, they have not been infinite as compared with American contributions, as the ratio of 3 to 0 would imply, and I think we ought to have one or two, to say the least, before England gets another. The fact that all other countries have one or more, and we have nothing is really something to take action upon. It is almost an insult to American electrical science that there is not any unit named after an American, and in the future, people will think there must have been some reason for it, that something must have been the matter with our electrical engineers at this time or they would have had a unit named after them or seen that there was a unit named after some American electrician. It is not only a slight to the men for whom the unit should be named, but it is a reproach to the men who do not make it their business to see that some unit is named after some American, and therefore I hope that this, or some equivalent resolution will be adopted.

DR. WHEELER:—I move the adoption of the resolution that the Secretary has read.

(The motion was seconded.)

DR. BELL:—I am heartily in accord with this resolution, but I must say that we may make our blanket so large that we shall find it covering some very undesirable things. In other words I feel very sure, and I think all my associates will agree with me in that, that if we pass this resolution and put the word "henry" into use to represent the unit of self-induction, it will be used in every case as the synonym of the present unit. In fact it has been so used since its first suggestion, and I believe if we ever make this like other names derived from the names of men, the name of a practical unit, we should endeavor to have it represent a unit which can be conveniently used in practice. I do not believe that any electrician ever had occasion to encounter the unit of capacity without indulging in animadversion on the men who got it out. We not only use microfarad, but the customary standard you find around laboratories is one-third of that. The earth's quadrant is unconscionably large. It makes even apparatus of high self-induction have a fractional value in terms of that unit. For example, we should have induction coils, etc., having fractional values of self-induction, say, .256 of a "henry." While we are selecting a practical unit, I think we should take a value which will be convenient in actual every day computation, so that when we are talking about it in a few years, we shall not be driven to using small fractions every time we want to express ourselves. I believe that rather than blanket the entire universe with a name, and letting it settle itself, we should confine it and push that definition into use as well as we can. If we do not confine it, it certainly will mean the present unit, which is not a practical unit at all. I believe that in pushing this resolution, we should make the "henry" a practical unit of say one hundreth the value of the present inconvenient absolute standard.

MR. PRESCOTT:—I would suggest that Dr. Bell present that in the form of an amendment.

DR. BELL:—Mr. President, I will make that as an amendment—that in passing this resolution, it is the sense of the Institute that this name should be applied to a practical unit, in value one one-hundredth of the earth's quadrant.

The motion was seconded.

MR. CROCKER:—I will suggest that this be added as another clause to this resolution so that it does not involve the rest of it. What is said in the resolution is that the name "henry" should be given to the practical unit of self-induction. That, so far as it goes, is all right.

DR. BELL:—I suggest one one-hundredth because I think it would fall more conveniently into practice. For example, I believe that the self-induction of the telephone instrument is somewhere about one one-hundredth of the quadrant, and the self-induction

for ordinary induction coils, for example, would be half a quadrant, and values of that sort. Now, if we took one-tenth of the practical unit which would be a million metres, we should still have fractional values for a large number of common instruments and common coils. It is for that reason I suggested taking a smaller figure. A thousandth might be practical, although it would make numbers somewhat larger. The one one-hundredth seems to be about the magnitude to make the self-induction which we ordinarily meet in practice, small whole numbers.

I am thoroughly in accord with Mr. Crocker's suggestion that this simply be added as a clause in the original resolution. I think it would serve the purpose there as well as anywhere else, because we simply fix the name "henry" and then recommend the practical unit, so that the two things would naturally go together.

Dr. Wheeler:—It seems to me that the work of selecting that fraction of the unit, which would be most convenient for use, is committee work, and it would be well to accomplish what Dr. Bell wants to accomplish by passing the first part of the resolution, and then making his resolution read, "And the selection of this unit shall be left to a Committee appointed by the Chair to report at the next meeting."

Dr. Geyer:—I think we ought to do that. The adoption of a name is one thing. The adoption of a magnitude is a very much more serious matter, and there are a great many serious considerations coming in. One one-hundredth of the present unit may be a convenient quantity for some. For others, another fraction may be a more convenient quantity, and there are different views to be taken of it, and I think those things ought notto be hurried.

The President:—It seems to me also that the unit as adopted should harmonize with the other practical units—the ohm, volt, ampere and so on, so that in computation it should not be necessary to use a multiplier in connection with the unit of self-induction.

Mr. Crocker:—That very harmonizing with the units already determined is just the reason why the farad has such an ungainly value. I think that if the farad had been made a millionth of its present value, in spite of its not agreeing with the volt and coulomb, it would have been very much more convenient. Not one man in ten thousand who uses the micro-farad knows or cares whether it agrees with the volt or the coulomb. The farad is simply that capacity which will hold a coulomb, at one volt pressure. That requires an almost infinite condenser. If it had been fixed at the capacity which would hold a thousandth of a coulomb, at a thousandth of a volt pressure, then it would be just a microfarad. I think that in practical units, the first thing to do is to have them of convenient size. Their derivation must take care of itself. Of course the value must be definite; and

we must know what it means. But it need not be based upon a ratio of 1 to 1. For one time that we have to derive it, we have to use it a thousand times, perhaps ten thousand. So that I think that practical units determined in the future should be convenient units, even though they are not derived from one volt and one ohm. Let them be derived from a tenth of an ohm or a volt, or a thousandth, if necessary, to make them convenient. This same question will come up very soon before electricians in regard to magnetic units. It is already coming up. It is recommended by Mr. Fleming to call one hundred million C. G. S. units of magnetic induction; that is one hundred million lines of force a "gilb." Now, one hundred million lines of force is a field produced by ten thousand square centimetres of cast-iron saturated. Ten thousand square centimetres is one metre square; that is, a square metre of cast-iron will produce one "gilb" when it is saturated. Now that is too large a unit, for the same reason that Dr. Bell has already stated—that every practical value would be a fraction. Dr. Fleming throws the "gilb" aside and begins right off with the "micro-gilb," and then he will probably get fractions of the "micro-gilb." In fact he uses one-tenth of a "micro-gilb." Now that is not a practical unit. I think that the units decided in the future should surely be made convenient. I would suggest that the resolution be adopted as read, with the additional clause that a Committee be appointed to determine the best and most convenient practical value for the "henry."

Mr. F. L. Pope:—It seems to me that the ultimate disposition we ought to make of this matter is to refer it to the International Electrical Conference, with our recommendation that the name "henry" be given to the practical unit of self-induction and our recommendation of a certain value. It does not seem to me that we can settle this thing all by ourselves. The rest would like at least to be consulted about it. I think if it were referred to them in that way there is very little doubt they could adopt it.

Mr. Crocker:—In regard to Mr Pope's remarks, there is a great deal of force in what he says, but the language of the resolution, I think, covers it. It says "that in our opinion it is proper that so and so should be done," which is a modest way of putting it. Then "that this Institute will gladly co-operate with other bodies in this country and abroad to secure its general adoption." If in addition to this Mr. Pope thinks we ought to say that the Institute recommends to the next International Congress of electricians that this unit be adopted by it, I see no objection to it. Would you put that in as an amendment?

Mr. F. L. Pope:—Yes, I will make that amendment and I think we can take action on that to-night.

Mr. Phelps:—The matter before the house now, as I understand it is the adoption of this resolution as it stands, is it not?

The President:—Unless this other motion is seconded.

Mr. Crocker:—I accept the amendment.

Mr. Phelps:—The amendment is to what effect?

THE PRESIDENT:—That it be referred to the International Congress or whatever body may be appointed to represent the International matters in regard to electricity.

MR. CROCKER:—The next International Congress.

MR. PHELPS:—That amendment I understand is accepted by the mover of the resolution so as to become a part of it.

THE PRESIDENT:—If the seconder agrees to that it may be counted part of the original resolution, if the house does not object to it.

MR. PHELPS:—Then I would offer a further amendment that a Committee of five be appointed by the Chairman to consider a proper magnitude for the unit "henry" and to make recommendations upon that subject to the next meeting.

(The motion was seconded.)

MR. CROCKER:—I accept that amendment.

THE PRESIDENT:—Is there any objection to its being incorporated as part of the original motion? If not it will be so incorporated. The motion is that we adopt this resolution which was read here in its printed form with the addition that we recommend its adoption to the next International Congress of Electricians and that a Committee of five be appointed to consider the best magnitude for the unit and to make recommendations and report at the next meeting. I believe we understand how the resolution reads. Those in favor of the resolution as stated please make it manifest by saying aye—contrary minds—no. It is carried unanimously.

DR. BELL:—Before taking an adjournment I would like to ask in behalf of the National Electric Light Association, for a little aid from the American Institute of Electrical Engineers. I chance to be the Chairman of the Committee of the National Electric Light Association to fix upon some definite classification of the lighting power of incandescent lamps. In other words, to determine what we should call a standard sixteen candle power lamp. A committee to straighten out the nomenclature of lamps was appointed at the last meeting of the National Electric Light Association, and feeling as I do the difficulty of the problem and the necessity of having it as thoroughly considered as possible, I would like to ask the appointment of a committee of three by the chair from this Institute to co-operate with the committee in that work. I offer the following:

Resolved.—That a committee of three be appointed to represent the American Institute of Electrical Engineers, in conference with a committee already appointed by the National Electric Light Association to fix upon a definite classification of the lighting power of incandescent lamps.

MR. CROCKER:—I would question whether that comes within our jurisdiction. That is purely a commercial question and we have never done much in that direction. But if the National Electric Light Association, which is especially interested in that subject, ask our co-operation, I certainly see no objection to giving it. But I think we better deliberate before we do go into purely commercial questions.

MR. PHELPS:—I wish to second Dr. Bell's motion and to say that I do not quite see the force of Mr. Crocker's point in regard to the irrelevancy of that topic to our work. I do not see why the Institute of Electrical Engineers might not properly have a word to say as to standards of lighting as well as standards of resistance or any other electrical matter, and as we have the request virtually from the National Electric Light Association—that is to say from the Chairman of one of its committees—I think it is but proper that we should lend a hand in that work. Therefore I second the motion.

DR. GEYER:—I think it is not out of our province to help to determine what a sixteen candle-power lamp is, especially as we have been asked.

THE SECRETARY:—One of the difficulties in settling upon standards is getting different bodies to unite upon them and it appears to me that this is a move in the right direction, because if the National Electric Light Association and the American Institute of Electrical Engineers work together and settle this question, it will have more weight than if either one undertook it separately.

MR. DOBBIE:—It seems to me that the question would simply resolve itself into a standard of light.

THE PRESIDENT:—Those in favor of the resolution to appoint a Committee to consider the classification of the lighting power of incandescent lamps will please make it manifest by saying aye: those opposed no. It is carried.

MR. MARTIN:—Was that the exact shape of the recommendation? I was present at the meeting at Cape May, and I did not quite understand that to be the shape of it. I understood it to include arc-lighting as well as incandescent. I may be wrong.

DR. BELL:—The Committee was appointed to consider only incandescent lamps. Of course that includes series incandescent lamps burned on arc circuits. The National Electric Light Association felt that it was undertaking quite too severe a task to straighten out the arc-lamps before disposing of the incandescent.

THE PRESIDENT:—How is this committee to be appointed?

SEVERAL MEMBERS:—By the chair.

THE PRESIDENT:—Then if you will allow me, I will appoint on the Incandescent Lamp Committee, Dr. Nichols, Dr. Duncan and Mr. Howell, and on the Committee for the consideration of the value of the "henry," Mr. Crocker. Dr. Geyer, Dr. Bell. Mr. Colby, and Mr. Phelps. Is there any further business?

On motion of Mr. Phelps the meeting adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Special Meeting, October 21st, 1890.

The meeting was called to order by President Anthony at 8.20 P.M.

The Secretary read the names of the newly elected associate members.

The Secretary:—The next business is the report of the Committee to determine the value of the "henry"; and the revision of the preliminary report of the wiring table committee is also to be acted upon.

Mr. Francis B. Crocker:—I can report for both committees. As I said at the last meeting, the reference of these matters to committees would probably involve a delay of several months. I have tried to have several meetings, without very much success. but we have about half done the work, I should say, and probably by next meeting we will be able to report the result in finished form. The question of the value of the "henry" is rather an important one, and if we are going to recommend a value different from that of the quadrant we want to be perfectly sure that we are right and that it will fit the requirements of practice. So I report progress for both committees.

The President: (Prof. W. A. Anthony)—The first paper for the evening will be on "Investigation of the Stanley Alternate Current Arc Dynamo," by W. B. Tobey and G. H. Walbridge.

A paper read at the 50th Meeting of the American Institute of Electrical Engineers, New York, October 21st, 1890, President W. A. Anthony in the chair.

INVESTIGATION OF THE STANLEY ALTERNATE CURRENT ARC DYNAMO.

BY W. B. TOBEY AND G. H. WALBRIDGE.

BEFORE entering directly upon the subject to be presented this evening, *i. e.*, the investigation of the Stanley alternate current arc dynamo, it may be well to give a brief description of that new and extremely novel type of machine. Descriptions of it have been given in some of the electrical papers during the past spring, but some here may not recall its general mechanical construction. The dynamo is manufactured and sold by the Westinghouse Electric Company under patents granted to Mr. Wm. Stanley, Jr., the inventor. Mr. Albert Schmid, superintendent of the Westinghouse Electric Company's shops, deserves much credit for perfecting the machine in detail, and to him is largely due its success commercially. In general appearance it closely resembles the Westinghouse alternate current constant potential dynamo, which has become so familiar to all. In fact, the frame, pole pieces, field windings, etc., for any given size machine are identical, whether for constant current or constant potential. The distinguishing feature between them lies wholly in the form and winding of the armatures. Figs. 12 and 13, showing diagrammatically half sections of the machine, will aid us in a description of the armature, which is built up of thin iron plates stamped in the form shown. It will be observed that a number of cores corresponding to the number of poles of the dynamo project radially from the armature, and from the sides of each core project overlapping lugs. The armature coils are not of the original "pancake," or oblong flat coil type, placed on the periphery of the armature, but are quite thick and placed around the core projections. The overlapping lugs serve the double pur-

pose of preventing the coils from slipping off the cores and also aid in the regulation of the machine. All the armature coils are connected in series and the terminals connected to the two collector rings, upon which bear the brushes furnishing current to the line. The field coils are also in series, so connected that the poles presented to the armature are of alternate polarity. The terminals of the field go directly to a small shunt wound exciter. Thus it is seen that the machine consists simply of an even number of field coils, corresponding to the number of poles on the machine, joined in series, and an equal number of armature coils also joined in series. That from such a simple combination of coils without any external regulating device, an almost constant current is obtained for all loads, from short circuit to

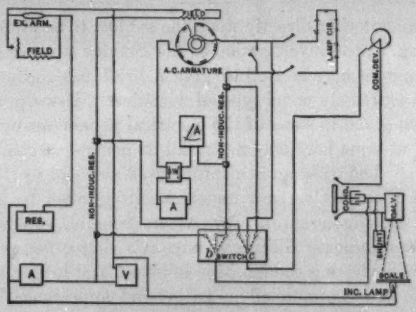

Fig. 1.—DIAGRAM OF CONNECTIONS.
A, A, A. Ammeters; V. Voltmeter.

the maximum output of the machine, seems incredible, but such is the case.

Through the kindness of Mr. Stanley, a complete 40-light plant was sent the writers at Cornell University, at which institution we were then students. Later, the Westinghouse Company made the University a gift of the entire plant. It has been our aim to show how the regulation takes place; also to determine the actual electrical output of the machine, and from the later and the corresponding dynamometer readings, to calculate the efficiency of the dynamo.

The alternator was mounted on a Brackett cradle dynamometer calibrated to read in watts direct at 1000 revolutions, the speed at which we were running.

The general method has been to obtain instantaneous values of E. M. F., arc and exciter currents at a sufficient number of points to indicate by curves the performance of the dynamo at every part of a revolution. For this purpose runs were taken with the dynamo at short circuit and with loads of 5, 10, etc., up to 40 lamps. The dynamo was run under normal conditions, viz , at a speed of 1000 revolutions and with such excitation as to cause an output of approximately 10 amperes. For obtaining values of E. M. F. at low loads, pressure wires were run from the terminals of the dynamo and for larger loads were taken from around 10 lamps. At all points where instantaneous values of E. M. F. were taken, corresponding values of arc and field, or exciter currents

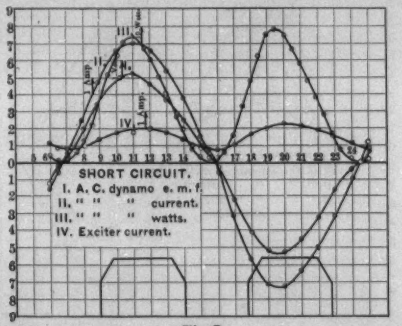

Fig. 2.

were obtained by "fall of potential" method. Pressure wires were brought from the terminals of each of two non-inductive resistances of german silver wire placed in arc and field circuits. (See Fig. 1.) These resistances being constant, any pressure around the terminals corresponded to a certain current. The terminals of the three pairs of pressure wires were connected to the contacts of the triple-throw switch, so that by placing the switch on any pair of contacts desired, those terminals could be put in circuit with a line in which was a condenser and the revolving contact or commutating device by means of which the condenser was charged. The revolving contact used was similar to the one described in detail by Prof. Ryan in his paper on "Transformers," read before the Institute last December.[1] It

1. See Transactions of the American Institute of Electrical Engineers, Vol. vii., p. 1.

consists essentially of a knife-edge of steel fastened by a wooden clamp to the collector end of the armature, and revolving with it through a circle of about eight inches diameter. The centre of the knife-edge was in line with the centre of one of the armature coils. Upon a frame at the same end of the armature was held a wooden semi-circle, upon the periphery of which was a paper scale, divided to millimeters. At the centre of the semi-circle was pivoted an arm carrying a brushholder in which was a steel spring. The brushholder was so adjusted that the revolving knife edge made contact with the spring several times per second. The arm carrying this spring could be clamped wherever desired,

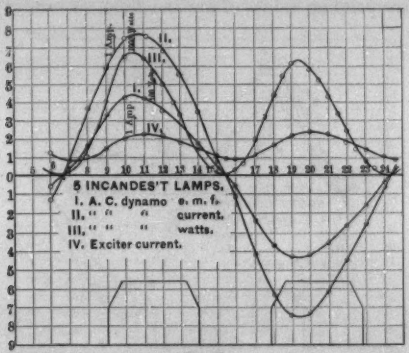

Fig. 3.

and its position designated by means of a pointer over the scale.[2] Thus, the condenser, placed in series, could be brought to the same difference of potential that existed between the spring and the knife-edge. The arc circuit was, as shown by the diagram of connections, Fig. 1, taken from one brush directly to a Westinghouse ammeter through a switch, by means of which a Thomson balance, or gravity ammeter could be put in circuit when desired. From the Westinghouse ammeter the current was taken through the non-inductive resistance to the dynamo short circuiting switch, where the other terminal of the machine was directly connected. The exciter current was measured by a Ryan gravity ammeter, and a Hartmann and Braun type voltmeter

2. Much trouble experienced by other investigators in this line, due to breaking of the spring, was entirely obviated by us by putting a piece of thin rubber packing above the spring where it was clamped in the brushholder.

indicated the potential around the terminals of the field. The arm of the commutating device being clamped at some desired position, the spring was adjusted so that the revolving knife-edge made contact, then the switch was thrown on any one of the three pairs of terminals, for the circuits of which we wished to obtain instantaneous values. The half microfarad condenser was so connected that by closing a key it would be charged. Opening this key and closing one in circuit with the galvanometer, sent the charge through the galvanometer and the throw was noted.

The galvanometer used was 5000 ohms resistance, and consisted of the coil of a Sir Wm. Thomson marine galvanometer, in which was suspended a small, concave mirror, having several

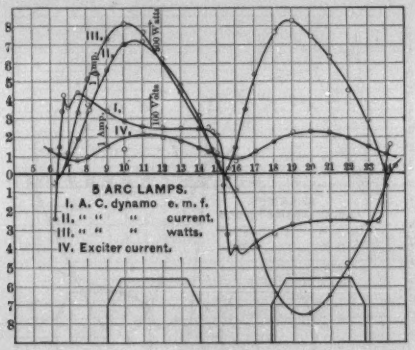

Fig. 4.

very small magnets constituting the needle, fastened on the back. The controller was a large horse-shoe magnet. A simple wooden frame supported the coil. A narrow slit in a screen, behind which stood an incandescent lamp, allowed an image of part of the filament to fall on the galvanometer mirror and be reflected back to the screen, on which was a graduated scale. By placing a resistance box in shunt around the galvanometer, the throw of the needle was reduced when it was desired to read the arc E. M. F. When current readings were taken, the whole charge of the condenser was passed through the galvanometer. The galvanometer was calibrated for E. M. F. by putting the condenser and a standard potential instrument in multiple around the terminals of a storage battery giving about 120 volts. Readings were then taken

of the voltage, and the corresponding throw of the needle was noted. The calibrations of currents were made by running the exciter alone and passing the current through both of the non-inductive resistances, determining its amount by an ammeter, and observing the corresponding throw of the galvanometer needle for each non-inductive resistance.

So much for a description of the dynamo, the instruments and the methods used. We now come to that which is of most interest, viz., the results obtained. In order that the curves may be more easily followed we will give a brief general explanation of them.

The figures on the zero line, always commencing with 5, cor-

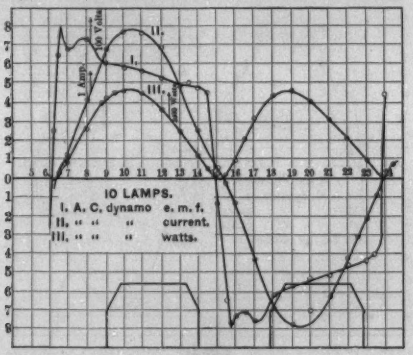

Fig. 5.

respond to the divisions on the scale of the wooden semi-circle, a part of the commutating device. Under each plate of curves will be seen two rectangular figures, representing pole pieces, which show the relative position of the curves with respect to the poles. From each curve projects a vertical arrow, always equal to one or two divisions, and its value marked near it. In all curves the direction or motion of the armature relatively to the poles is from left to right.

Fig. 2 represents the performance of the machine at short circuit. The curves of E. M. F. and current both approach sine curves; the current rising almost in unison with the E. M. F., there being no perceptible lag. The curve of exciter current in

this, as in all cases, exhibits a wavy form. Fig. 12 shows the position of the armature relatively to the poles when the E. M. F. is zero and the machine running on short circuit.

Fig. 4, representing the curves for a load of five arc lamps, shows a radical change in the E. M. F., which now approaches more nearly to the form of a rectangle than a sine curve, and immediately after passing the zeros exhibits very sharp peaks. In this case the current continues to rise after the E. M. F. has begun to fall, and also lags slightly with respect to it. A shifting of the zeros has also taken place. Comparing this E. M. F. curve with that for short circuit it is seen that the first zero shown occurs at about 6.35 instead of 6.85. The curves for all loads of

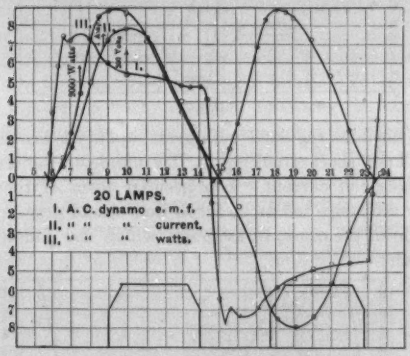

Fig. 6.

arc lamps are quite similar in form. as will be seen by referring to Figs. 5, 6, 7 and 8. As the load is increased, however, the zeros occur earlier with respect to the poles, and the current lag increases slightly. The efficiency curve, Fig. 9, tells the story of the dynamo from a commercial point of view, reaching as it does an efficiency of between 93 per cent. and 94 per cent. at full load while at half load it is 92 per cent.

A most interesting set of curves is shown in Fig. 10, giving a comparison of the forms and values of E. M. F. for 40, 30, 20, and 10 lamps, their position with respect to the poles, and also the position of the zeros of E M. F. at short circuit.

Figs. 13 and 12 show respectively the positions of the armature relatively to the poles for full load and short circuit.

An apparently remarkable feature of the machine is that it may be short circuited when running at full load, without the slightest injury, and it may be run at short circuit indefinitely.

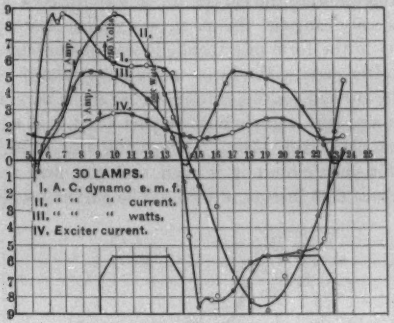

Fig. 7.

That the machine regulates with great rapidity is proven by the fact that upon short circuiting it, the ammeter indicates but a slight increase of current. The peculiar form of the E. M. F. curve for arc lamps led us, so far as time would permit, to an investigation of the cause, to ascertain which the following runs were taken:

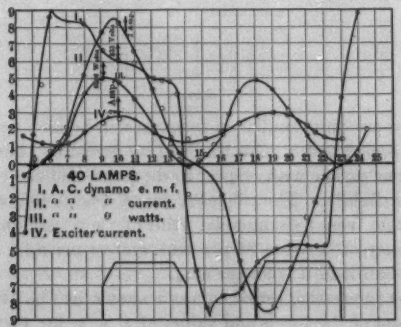

Fig. 8.

First, with a non-inductive resistance of five large incandescent lamps, but no arcs in circuit; each lamp consuming about the same amount of energy as an arc lamp. Second, with noth

ing but an inductive resistance, consisting of a solenoid having 440 turns of No. 10 wire. The curves in both cases were similar and approached sine curves, proving the rectangular form to be

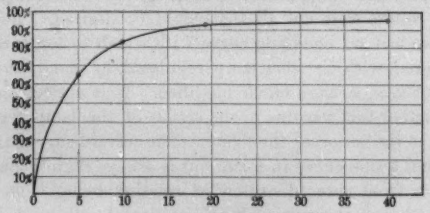

Fig. 9.—EFFICIENCY CURVE.

due entirely to the action of the arc. Fig. 3 shows the curves obtained from the load of incandescent lamps. To more fully determine this action with no other resistance in circuit, an arrangement was made whereby an arc could be regulated by hand with the aid of a hot wire voltmeter. We took no curve for a single lamp under normal conditions with which to compare the one taken in the above manner; but the latter (see Fig. 11) has very high and abrupt elevations of E. M. F. immediately before and after passing the zeros. The theory advanced to explain

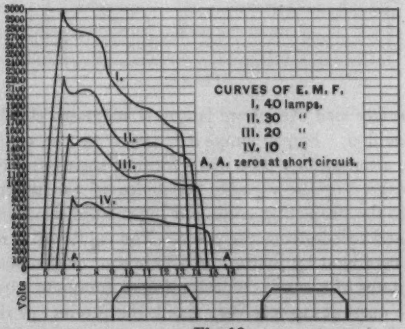

Fig. 10.

the high point of E. M. F. in all the arc curves is, that when the arc breaks, the rush of cold air adding resistance to the circuit, prevents the arc forming again until the potential has risen to a

point considerably higher than that required to maintain an arc once formed. To test this we attempted to heat an arc by means of an alcohol blast, and thus prevent, in a measure, the rush of cold air. This means of heating proved inadequate, so that the results were not all that could be desired. As before stated, we had no one-lamp curve with which to compare it, but as compared with the one lamp regulated by hand, the sharp corners of E. M. F. were much modified. It is not certain, however, whether this was due to the action of the lamp regulating coils, or to the action of the flame. The current curve in all cases approaches a sine curve in form, but is always slightly steeper on the left or front side; direction of rotation being from left to right. Although the current lag is practically nothing at short cir-

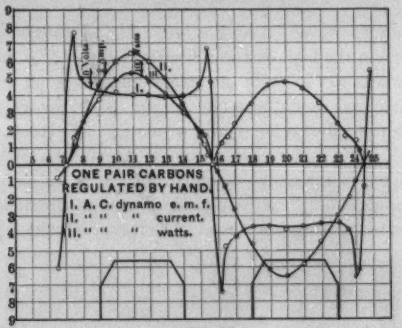

Fig. 11.

cuit, or for any load except arc lamps, it increases slowly as arc lamps are added. For 40 lamps it is about $4\frac{1}{2}$ per cent. of the time of an alternation. The wavy form of the exciter current is probably caused by the variable resistance of the magnetic circuit, due to the projecting cores and lugs of the armature as they approach and recede from the poles. The values of E. M. F. and current were found by taking the square root of the mean square of 36 ordinates of a curve, representing a complete circle. Although the current lag was very small, the product of the values of current and E. M. F. obtained as above did not give the watts. The watts were obtained by plotting a curve, the points of which were the products of the instantaneous values of E. M. F. and current; then integrating by a planimeter. Dividing the value

of the watts thus obtained by the current, we obtained a value of the E. M. F. which was usually about seven volts lower per lamp than the corresponding value obtained from the square root of

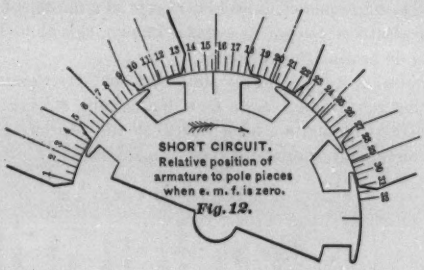

Fig. 12.

the mean square. This difference was no doubt due to the extremely high values of the E. M. F. when the current was very low, as the negative work was negligible. To distinguish between the two values we have called the one obtained from the square root of the mean square, the "actual" E. M. F. while the one obtained by the second method has been called the "effective" E. M. F., it alone being effective in doing work on the circuit.

It is very much of a question in our minds as to whether the square root of the mean square, obtained from the rectangular E. M. F. curves for arc lamps, gives the actual E. M. F., but for the

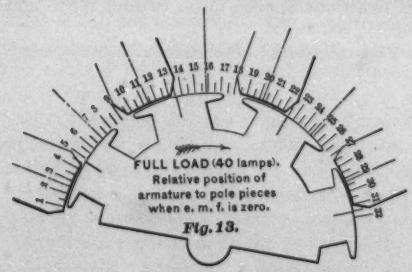

Fig. 13.

lack of a better term, the values thus obtained have been called the actual E. M. F's.

For arc current values we have assumed the square root of the

mean square to be correct, and if not actually the case, they are certainly very close approximations, as the current curves are practically sine curves. The exact values of E. M. F. or current are of little consequence, however, except as a matter of interest, since the electrical output in watts is known, this alone being required to determine the efficiency.

In conclusion we can simply state that our investigations have shown, that the dynamo, aside from being very efficient and of most excellent regulation, is, as stated at the beginning, an extremely novel and interesting type of machine.

TABLE OF DATA.

	√Mean sq. of E. M. F.	Volts per lamp "actual."	√Mean sq. of Current.	Volts per lamp "effective."	Mean watts electrical.	Watts excitation.	Mean watts by dynamometer.	Efficiency.
Short Circuit.	3.53		9.78		35	155	1780	
5Incandescent.	289.	57.8	10.39	59.3	3060	158	3900	75.3%
5 Arc.	285.	57.	9.98	50.	2500	165	3584	66.6%
10 Arc.			10.7	53.3	5700	190	6630	83.5%
20 Arc.	1100	55.	10.36	48.3	10000	190	10680	92.3%
30 Arc.						204	12120	
40 Arc.			9.82	41.	16000	240	16950	93.2%
1 pr. carbons.	42	42.	8.86	35.5	314			

THE PRESIDENT:—As the paper of Mr. Reid applies in the same direction, that will be read before discussion is called for. The next paper will then be "A New Method of Analyzing Armature Reactions, applied to the Stanley Arc Light Alternating Current Machine."

A NEW METHOD OF ANALYZING THE MAGNETIC REACTIONS OF ARMATURES APPLIED TO THE STANLEY ARC-LIGHT ALTERNATING CURRENT MACHINE.

BY THORBURN REID.

This analysis is a result of looking at an old and well-known phenomenon from a new point of view, and is merely a development of the principle of the magnetic circuit a little further than has heretofore been usual. Hopkinson, I find, has used much the same method in his analysis of the transformer, and seems vaguely to have foreseen its application to dynamos; if I may judge from a part of a sentence which I ran across in one of his articles a few days ago, this same principle has been discussed at some length in the English electrical journals lately, but I am not aware that any analysis has been attempted by means of it.

By this conception dynamos are considered to consist of a magnetic circuit composed partly of iron (or steel) and partly of air, one part being at rest and the other in motion (generally rotary). Magnetism is induced in this circuit by means of two or more coils of wire carrying an electric current, wound around some part or parts of the circuit. One or more coils may be wound on the fixed part of the circuit and one or more on the moving part, and the magnetism induced will be that due to the sum of all the magneto-motive forces in all the coils, divided by the sum of all the magnetic resistances. The magneto-motive forces in the fixed coils are generally approximately constant, except where they are varied for purposes of regulation. In the movable coils, however, the M. M. F. is generally variable. In direct current machines its effect on the total flux around the magnetic circuit is also almost inappreciable, chiefly for the reason that it acts almost at right angles to the magnetic circuit. In alternating current machines,

however, it is quite considerable, and is generally taken into account, but attributed to the self-induction of the armature. The connection between the counter M. M. F., as it may be called, of the armature and its self-indnction will be brought out more fully in a subsequent part of this paper.

To simplify our conceptions we will assume a two pole dynamo with one coil on the armature, as shown in Fig. 1. Referring to the diagram, Fig. 2,

Let $\overline{ON} = N$, represent the magnetic induction through the armature due to the field in magnitude and direction.

Let $\overline{OC'}$ represent the position of the armature coil at that instant.

Let $\overline{OA} = A$, at right angles to $\overline{OC'}$, represent the magnetic induction due to the armature coil at that instant.

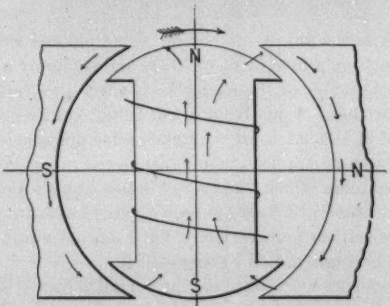

Fig. 1.

Let $\overline{PP}$, at right anges to the field lines, be the neutral line.

Let $POC' = \theta$ be the angle through which the coil has moved from its neutral position.

By completing the parallelogram, and thus compounding the two magnetizations N and A, we obtain $M = \overline{OR}$ as the resultant magnetic induction through the armature coil in amount and direction at that instant, and $\overline{QQ'}$, at right angles to $\overline{OR}$, as the neutral line with reference to this resultant magnetization. Then QOC $(= \sigma)$ is the angle through which the coil has turned from this new neutral line.

Let the angle $\overline{NOR}$ $(- ORA) = \alpha$. We are to deduce the value of M from the known quantities N, A, and the angle θ. In the triangle $\overline{ROA}$, $\overline{RO}$ is perpendicular to $\overline{OQ}$ and $\overline{OA}$ is perpendicular to $\overline{OC}$; therefore the angle $\overline{ROA} = QOC = \alpha$.

Also $\overline{OA}$ is perpendicular to $\overline{OC}$ and $\overline{AR}$ is perpendicular to $\overline{OP}$; therefore $\angle OAR = \angle COP = 180° - \theta$.

From trigonometry we have:—

$$\frac{M}{\sin(180° - \theta)} = \frac{V}{\sin\sigma} = \frac{A}{\sin\alpha};$$

or

$$\frac{M}{\sin\theta} = \frac{N}{\sin\sigma} = \frac{A}{\sin(\theta - \sigma)} \qquad (1)$$

therefore

$$N\sin(\theta - \sigma) = A\sin\sigma.$$

Expanding $\sin(\theta - \sigma)$, dividing by $\cos\sigma$ and solving for $\tan\sigma$, we have

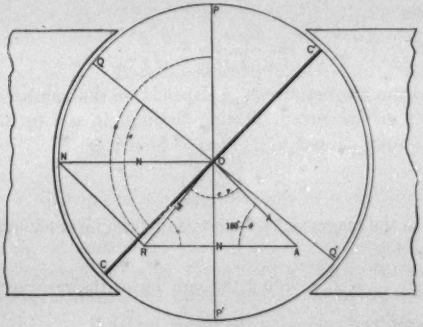

FIG. 2

$$\tan\sigma = \frac{N\sin\theta}{N\cos\theta + A} \qquad (2)$$

whence by trigonometry,

$$\cos\sigma = \frac{N\cos\theta + A}{\sqrt{N^2 + 2AN\cos\theta + A^2}} \qquad (3)$$

$$\sin\sigma = \frac{N\sin\theta}{\sqrt{N^2 + 2AN\cos\theta + A^2}} \qquad (4)$$

Therefore from equation (1)

$$M - N\frac{\sin\theta}{\sin\sigma} = \sqrt{N^2 + 2AN\cos\theta + A^2} \qquad (5)$$

This last expression gives us the actual value of the induction

through the armature coil at that instant in terms of the field strength, the armature strength and the angle θ through which the coil has been turned from the position perpendicular to the field lines. The direction of this resultant magnetization is along $\overline{OR}$, and the neutral line has been shifted from $\overline{OP}$ to $\overline{CQ}$ and the number of lines actually passing through the coils is $M \cos \sigma$.

From equations (3) and (5) we obtain $M \cos \sigma = N \cos \theta + A$. The E. M. F. at any instant is then

$$E = -\frac{d}{dt}\left\{ S\,N \cos \frac{2\,\pi\,t}{T} + S\,A \right\} \tag{6}$$

where S is the number of turns in the coil; t is the time which has elapsed since the coil passed the position OP, and T is the time of one revolution.

Therefore

$$E = \frac{2\,\pi\,S\,N}{T} \sin \frac{2\,\pi\,t}{T} - S\frac{d\,A}{d\,t} \tag{7}$$

The armature magnetization, A depends on the number of turns in the coil, on the current flowing through it, and on the magnetic resistance offered to its lines of force; or

$$A = \frac{S\,i}{\rho} \tag{8}$$

where ρ to the magnetic resistance, and i the instantaneous value of the current.

Letting $\frac{S^2}{\rho} = L$, dividing through by R the resistance in the main circuit, and collecting the terms, we have

$$\frac{di}{dt} + \frac{R}{L}i = \frac{2\,\pi\,S\,N}{L\,T} \sin 2\,\pi\,\frac{t}{T} \tag{9}$$

Let

$$\frac{2\,\pi\,S\,N}{L\,T} = a, \frac{2\,\pi}{T} = p, \text{and } \frac{R}{L} = b,$$

and we have

$$\frac{di}{dt} + b\,i = a \sin pt \tag{10}$$

a differential equation which must be integrated.

The equation may reduced to the following form

$$i = e^{-bt} \int a \sin pt\, e^{bt}\, dt \tag{11}$$

which may be integrated by parts thus

$$i = ae^{-bt} \int e^{bt} \sin pt\, dt.$$

Let $u = \sin pt$ and $dv = e^{bt}\,dt$; then $du = p \cos pt\,dt$ and $v = \frac{1}{b}e^{bt}$; $\int u\,dv = \frac{1}{b}e^{bt}\sin pt - \int \frac{1}{b}e^{bt}\,p\cos pt\,dt$;

$$i = \frac{a}{b}\sin pt - \frac{ap}{b}e^{-bt}\int e^{bt}\cos pt\,dt.$$

Let $u = \cos pt$, and $dv = e^{bt}\,dt$; then $du = -p \sin pt\,dt$, and $v = \frac{1}{b}e^{bt}$; $\int u\,dv = \frac{1}{b}e^{bt}\cos pt + \int \frac{1}{b}e^{bt}\,p\sin pt\,dt$;

therefore $i = \frac{a}{b}\sin pt - \frac{ap}{b^2}\cos pt - \frac{p^2}{b^2}i$; or

$$i = \frac{a}{\sqrt{p^2+b^2}}\left\{\frac{b}{\sqrt{p^2+b^2}}\sin pt - \frac{p}{\sqrt{p^2+b^2}}\cos pt\right\}$$

Let $\tan^{-1}\varphi = \frac{p}{b}$. Then substituting for a, b and p their values we have

$$i = \frac{2\pi SN}{\sqrt{R^2T^2+4\pi^2L^2}}\sin(\theta-\varphi) \qquad (12)$$

where φ is the angle of a lag, or the angle by which the effective E. M. F. lags behind the impressed E. M. F.

This is the instantaneous value of the current at any instant. Since the average value of the square of the sine of any angle is $\frac{1}{2}$, we have, calling C the square root of the average square of the current:

$$C = \frac{\sqrt{2}\,\pi SN}{\sqrt{R^2T^2+4\pi^2L^2}} \qquad (13)$$

$$E = \frac{\sqrt{2}\,\pi SNR}{\sqrt{R^2T^2+4\pi^2L^2}} \qquad (14)$$

You will no doubt have already recognized this equation as the ordinary equation of the alternating current dynamo, *when the self-induction of the armature coils is taken into account*. This analysis so far has taken no account of the self-induction in the armature coil. There has, however, been introduced into the discussion a factor not generally treated in this connection, namely, the counter M. M. F. of the coil. This factor occurs in the equation as the expression for which I have used the letter L. L is ordi-

narily used to denote the co-efficient of self-induction, and I chose it for that very reason. The connecting-link is shown by the value given to L in our equation; namely $\frac{S^2}{\rho}$.

We shall now show that L, the co-efficient of self-induction of a coil, is equal to the square of the number of turns in the coil, divided by the magnetic resistance of the circuit, through which the lines of force of the current flowing in the coil must travel. There are several ways of getting at a definition of L and perhaps the simplest is the following:

The counter E. M. F. of self-induction of a coil, in which a varying current is flowing, is $L\frac{di}{dt}$ where $\frac{di}{dt}$ is the rate of change of the current at the instant considered; or, more simply, if the current is changing at a constant rate, it is the number of amperes through which the current is increasing or decreasing in a second. If the current is changing at the rate of one ampere in a second, $\frac{di}{dt}$ becomes unity and $L\frac{di}{dt}$ becomes equal to L. Therefore L represents the counter E. M. F. of self-induction of a coil, in which the current is changing at the rate of one ampere in a second.

Now selecting suitable units, the number of lines of force which will be sent around a magnetic circuit of magnetic resistance ρ by a current of one ampere flowing through a coil of S turns, will be $\frac{S}{\rho}$. Therefore if the current is increasing at the rate of one ampere per second, $\frac{S}{\rho}$ lines of force will be inserted or taken out of the coil in a second, and the E. M. F. set up will be $\frac{S}{\rho} \times S = \frac{S^2}{\rho}$; but, since the current is changing at the rate of one ampere per second, the E. M. F. set up $= L$. Therefore $L = \frac{S^2}{\rho}$.

These equations (13 and 14) might have been arrived at a little differently by resolving the counter-magneto-motive-force of the coil parallel to the induction due to the field, and then considering the comparative values of the resistances through the inside of the coil and around the outside; but this method is more complicated than the one I have chosen, and a little consideration will show that it is precisely the same thing, merely looked at from a different point of view.

Now let us see how this equation must be modified to suit the conditions obtaining in the Stanley alternating arc machine. This machine is of the multipolar type, with the coils laid flat on the armature core. It differs from the ordinary Westinghouse alternating machine mainly in the facts that each coil consists of a large number of turns, and is laid in longitudinal grooves in the armature core in such a manner as to be almost entirely surrounded by iron, and that the number of alternations per second is large. As a result of this arrangement the machine maintains a nearly constant current throughout wide variations of resistance and speed.

To explain this phenomenon we will use equation (13), first substituting for L its value, $\frac{S^2}{\rho}$, and we have

$$C = \frac{\sqrt{2}\ \pi S N}{\sqrt{R^2 T^2 + 4\pi^2 \frac{S^4}{\rho^2}}} \tag{15}$$

Since there is a large number of turns in the armature coil, S is very large and S^4 correspondingly larger. Then, since the coil is almost completely surrounded by iron, ρ is very small, and accordingly $\frac{S^4}{\rho^2}$ is exceedingly large. Then, since the number of alternations is very large, T, the time of one alternation, is a very small fraction, and, therefore, $R^2\ T^2$ may be neglected as compared with $4\pi^2 \frac{S^4}{\rho^2}$. Neglecting this term then and reducing, our equation becomes.

$$C = \frac{N \rho}{\sqrt{2}\ S} \tag{16}$$

This equation shows that C is dependent on the magnetic induction through the coil due to the field, on ρ the magnetic resistance to the lines of force of the coil, and on S, the number of turns in the coil. *Resistance and speed are conspicuous by their absence.*

We must remember, however, that if R and T are increased sufficiently in value, $R^2\ T^2$ will cease to be negligible as compared with $4\pi^2 \frac{S^4}{\rho^2}$, and therefore, every increase in R will increase the denominator of equation (15) and C will decrease.

If, therefore, we fix the limits between which the current may change, while the resistance in the main current is changing from

its lowest to its highest value, we may determine the speed below which the machine must not run. Any increase of the speed above this point merely tends to lessen the fluctuation of the current between the highest and lowest resistances. Again, any increase in the speed above this point will widen the range throughout which the resistance may vary, without the fluctuation in the current passing the prescribed limit. R will have its lowest value when the armature is short-circuited, and will then equal the resistance of the armature coils, and the current will be at its maximum value The minimum value of the current then being fixed, the maximum value of R will depend on the value of $R^2 T^2$, as compared with $4 \pi^2 \frac{S^4}{\rho^2}$ or R (maximum) may vary inversely with T, or directly as the number of alternations per second.

Returning now to equation (16) we see that C varies directly as N (the field is separately excited) and N varies directly as the exciting current. Therefore any increase in the magnetizing current will correspondingly increase the main current. This is in fact the method by which the main current is brought to its proper value. The machine is short-circuited and the main current brought to its proper value by varying the magnetizing current. The main current will then remain practically constant throughout the prescribed range of resistance.

Secondly, the current varies directly as ρ. This, too, has been shown to be true, for in one of the first machines built the coils were wound in cylindrical logitudinal holes in the armature core so that they were entirely surrounded by iron, and as a result the current obtained was too small to be measured. Longitudinal slots were then cut in the armature core so as to increase ρ, and by widening these slots to the proper amount, the desired current was obtained.

Finally C varies inversely as S, the number of turns in the armature coil. That is, the more turns in the armature coil the less the current delivered. This, too, was found to be true in the experimental stage of the work. But it must be remembered that S must not be made so small that the term $4 \pi^2 \frac{S^2}{\rho^2}$ becomes comparable with $R^2 T^2$. We may perhaps obtain a simple conception of the reaction by slightly changing the form of our equation We may write equation (13) thus.

$$C = \frac{\sqrt{2}\,\pi\,S\,N}{R\,T\sqrt{1+\frac{4\,\pi^2\,L^2}{R^2\,T^2}}} = \frac{\sqrt{2}\,\pi\,S\,N}{R\,T\,\sqrt{1+\tan^2\varphi}} \qquad (17)$$

where φ is the angle of lag.

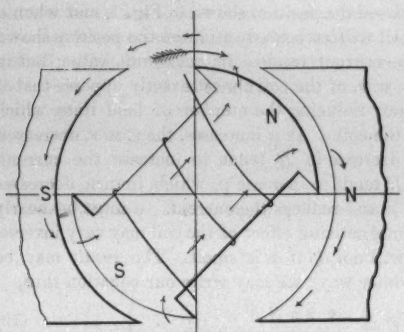

Fig. 3.

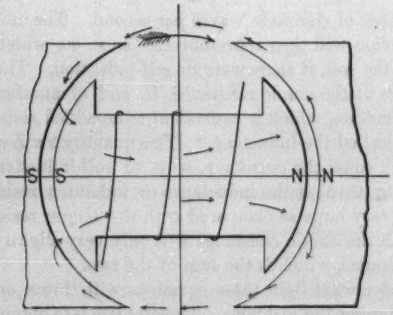

Fig. 4.

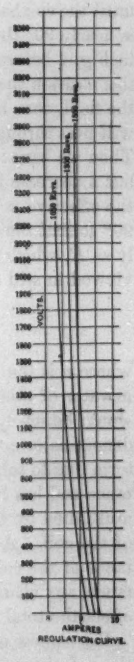

Fig. 5.

Neglecting the term 1, under the radical sign, we have

$$C = \frac{\sqrt{2}\,\pi\,S\,N}{R\,T\tan\varphi} \qquad (18)$$

tan φ varies inversely as $R\,T$, and therefore the product of tan φ and $R\,T$ in the denominator remains constant. Referring to Fig. 1, and remembering that φ is the angle by which the effective E. M. F. lags behind the impressed E. M. F., we observe that if there

is no lag in the effective E. M. F., or, the current will reach its maximum value when the coil is in the position shown in Fig. 1; and therefore the largest possible number of lines will flow through the coil and will be taken out of it during the revolution. As φ increases, the current is delayed in reaching its maximum value, until the coil has passed the position shown in Fig. 1, and when φ is nearly 90° the coil will have nearly attained the position shown in Fig. 4 before the current reaches its maximum value; but in this position the M. M. F. of the coil almost exactly opposes that of the field, thus largely reducing the number of field lines which will pass through the coil. As φ increases, the E. M. F. decreases. Then we have: a decrease in R tends to increase the current, but a decrease in R tends to increase φ, which, in turn, decreases the effective E. M. F. and reduces the current. φ must be nearly 90° so that the demagnetizing effect of the coil may vary inversely as R, which it will not do if φ is small. The result may be viewed in still another way; we may write our equation thus,

$$C = \frac{\sqrt{2}\,\pi\,S\,N\,n}{\sqrt{R^2 + 4\,\pi^2\,L^2\,n^2}} \qquad (19)$$

where n is the number of complete waves per second. The numerator of this fraction will then represent the E. M. F., which would be set up in the coil, if there were no self-induction. The denominator consists of the ohmic resistance R, and of another term due to self-induction, which is equivalent to an added resistance and has been called the inductance. This quantity $2\pi L n$ multiplied by C will equal the counter E. M. F. of self-induction of the coil. As long, then, as the inductance or induction resistance of the coil is very large as compared with the ohmic resistance, any change in the ohmic resistance will only very slightly affect the total resistance, which is the sum of the two.

Let us now consider what light these equations will throw on the question of designing the machine. The problem is to obtain a current of the desired strength and constancy, with the least expenditure of energy and at the least expense, and the quantities with which we may work to obtain this result are $N\,S\,\rho$ (or L) and T. We see immediately that it is advantageous to make T as small as mechanical considerations will allow, since the smaller T is, the more constant will the current be throughout the range, or the wider the range the resistance may have without causing the current to vary below the prescribed limit. This points then to a multiplicity of poles, or high speed, or both.

N, on the other hand, should be as large as possible, for the output of the machine varies directly with that factor, and its increase does not at all affect the fluctuation of the current. S. again, should be as small as it can be made without affecting too much the regulation of the machine.

ρ is the one indeterminate quantity in this equation. It is almost impossible to calculate its value beforehand to any reasonable degree of accuracy, for several reasons. The magnetic circuit of which it is the resistance, varies in cross-section through-

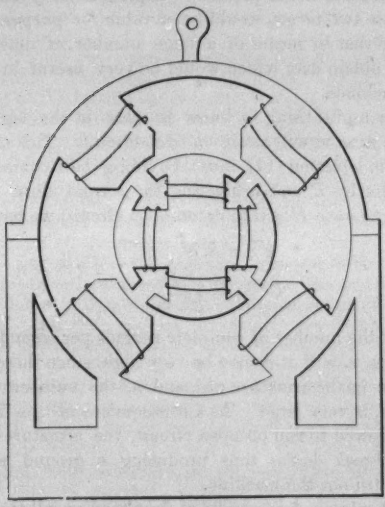

Fig. 6.

out its whole length, and this variation is not uniform but has several points of discontinuity. Furthermore, ρ varies during the revolution of the armature, for when the armature pole is half way between two field poles, part of the path of the lines of force is through the field pole, and when the armature pole is opposite the field pole, that same part of the path of the lines of force will be made up of the air gap between the two field poles, and ρ will be much increased. An approximate value might be obtained by determining L with the armature in different posi-

tions and taking an average of the values thus obtained; but if in actual practice the iron is worked anywhere near the saturation point, this method would be of doubtful utility, unless the currents used in determining L were the same as those used in practice.

Probably the best method for practical work would be by means of a curve of current and E. M. F., where, all the terms being known except ρ and probably N, we can obtain $n\rho$ from one point on the curve, and both ρ and N from two points. This method, while it would probably not give us very accurate values of these two terms, would be of value for purposes of comparison, so that by means of a large number of determinations we might obtain data which would be very useful in designing future machines.

Another useful thing to know is what is the highest value which the E. M. F. will attain on open circuit. This may be obtained from equation (14) thus: Dividing both numerator and denominator by R and neglecting the second term under the radical sign (since R is infinite on open circuit) we have,

$$E = \frac{\sqrt{2}\,\pi\,S\,N}{\sqrt{T^2 + \frac{4\,\pi^2\,L^2}{R^2}}} = \sqrt{2}\,\pi\,S\,N\,n \qquad (20)$$

where n is the number of complete periods per second.

This E. M. F. will of course be very high, since there are very many turns in the armature coil, and n, the number of periods per second, is very large. As a consequence of this, if the machine is allowed to run on open circuit, the armature insulation will soon break down, thus producing a ground which will speedily burn out the machine.

The following is the complete formula where all the terms are expressed in practical units.

$$C = \frac{\sqrt{2}\,\pi\,S\,N}{10\sqrt{10^{18}\,R^2\,T^2 + 12.768^2\,\pi^3\,S^4\,\mu^3\,\frac{A^3}{l^3}}} \qquad (21)$$

Discussion.

The President:—I think the members of the Institute will agree with me that these are two exceedingly interesting papers. We should be very glad to have a full discussion.

Mr. William Stanley, Jr.:—Mr. President, the papers that have been read this evening refer to the regulation of a dynamo with which there is no external appliance to control the current. In applying an alternating potential to a circuit, there are two kinds of opposition to current flow. One kind is offered by the resistance of the circuit, determinate in the dimensions of the circuit, and the specific material of which it is composed, while the other is the opposition due to the shape of the circuit, that which we ordinarily call the self-induction of the circuit. Now the relation of these two oppositions to the flow of current is not exactly the same. The resistance of the circuit opposes the flow of the circuit at the instant the potential is applied. The counter-electro-motive force of self-induction first delays the flow of current and then urges it on, and the time that elapses between the application of a given potential to a circuit and the flow of current corresponding to any particular value of the potential, is determinable by the ratio of the value of the self-induction to that of the resistance. Now in this machine that these gentlemen have spoken of, we may consider that all that part of the circuit which is inside of the machine, viz., the armature coils, represents the self-induction part of the circuit. This portion therefore tends to delay the flow of the current for awhile, and afterwards to push it on. All that part of the circuit external to the machine may be considered to represent the resistance portion of the circuit. Now it is evident we cannot change the number of turns of wire, or very well the number of revolutions of the machine when running; consequently we cannot change the value of the self-induction in circuit and therefore the ratio of the self-induction to the resistance. The change of this ratio—by simply short-circuiting such arc-lamps as we may wish to turn out, changes the time of appearance of the current. It lets the armature of the machine get a little further ahead when one lamp is burning, when the maximum value of the current in the armature occurs; that is, it allows the armature pole to come up in front of the field pole, as shown in Fig. 10, and to oppose the field pole, (the magnetization in the armature being directly opposite to that in the field) it keeps the lines of the field magnetism from passing through the armature. This effect is very beautifully shown by winding on the two armature circuits, one an arc circuit, and another a few turns of wire connected with the voltmeter, both wound upon the same tooth. If the machine be short circuited, the magnetism is, as it were, blown out, and no potential is found upon the supplemental circuit. The whole regulation of the machine is due then to the fact that, as shown in Fig. 10, the time of the current flow has advanced, as we see

it, from the point AA of the horizontal line to the point near the numeral 5—13¾. If we could put upon the field of the machine a separate circuit and as fast as we turned out the arc-lamps that we wished to compensate for, put a current through that circuit decreasing the field strength, we would obtain exactly the same results the machine attains by its own armature action.

MR. A. E. KENNELLY:—I think that the paper which we have heard from Mr. Reid was an interesting one. It contains a great deal of matter in a short space, and I confess that personally I have not been able to fathom it completely so as to deal with it upon the merits that it undoubtedly possesses. But, if I mistake not, I have grasped the general idea that we have before us—a method which is comparatively new, although not entirely so, of considering inductance in a dynamo not as a geometrical constant belonging to the wire or to the circuit, but as a function of magnetic resistance. Of course the two things are inseparably associated. The ordinary method, which is the shape method, which is supposed to be connected with the fact that an inductance is necessarily a length, is the original method. Any one who has tried to determine what the inductance of a coil is on theoretical grounds, or who has endeavored to follow the first formula given by Neumann for that determination which deals with an integral of little elements of length taken all around the circuit, each little part acting on every other part at a certain distance supposed to be measured or known, and then multiplied by the cosine of the angle between those two elements, must feel that this treatment is exceedingly artifical and almost unnatural. He feels that although it yields a result which is correct, there is something there which suggests action at a distance, and not in the way in which we believe nature herself must act. The other way, which deals with magnetic resistance is a much more tangible one. It supposes a certain stress brought on the material, the resistance which that matter opposes to the magnetic force and the total effect of that resistance on the magnetic flux. The magnetic flux itself for unit current is then the inductance in the ordinary sense of the term. Consequently any method which will enable us to determine inductance from a simple conception of magnetic resistance must be immensely valuable, not only theoretically but also for the treatment of the most intricate problems which often present themselves for analysis. In continuous current machines, as Mr. Reid has pointed out, these reactions are few, comparatively simple, and more easily determined. In alternating current machines there are problems which baffle at present all the intelligence which can be brought upon them. Any means by which we can take a coil with any specified magnetizable environment and determine what the magnetic resistance of that circuit of environment will be, is most desirable and I think that any one who looks on this problem, however sadly he turns away from the fact that it is still a generally un-

solved problem, must feel that there must be some means of attainment to this end, and of following out that plan by analysis.

I can only regret that such papers as these which contain so very much food for reflection cannot be given to us in advance, so that we could be better able to discuss them.

MR. R. W. POPE:—With regard to the last suggestion of Mr. Kennelly, I would like to have him go a little further and inform the Secretary how he can attain that much to be desired end. I have been laboring to do it for the last three years and I have succeeded about three times, I think. That was at our meeting last spring when I secured three out of nine papers and those were printed in advance of the meeting. I can see no plan unless I undertake to write the papers myself.

MR. THOMAS D. LOCKWOOD:—Having come three hundred more or less miles to attend this meeting, I find that words fail me when I undertake to express my appreciation of the papers which have been presented. I had some trouble at first in finding which side of my paper was up, but having found it I had no difficulty in following the reading of the two papers. I think the Institute is very much indebted to the gentlemen who give us papers prepared with so much apparent carefulness as these, to think of afterwards, for I apprehend that the value of these papers lies, chiefly in the fact that they will be printed and that we shall have them to read and to study in after years. The first paper, I suppose, may be regarded as being an eminently practical one, and I have no doubt that to those who are engaged in the actual practice of electric lighting by alternating currents it will be of the greatest utility. I for one am very glad to see such papers presented. The second, which is far more theoretical in its nature, of course, is the one which I had chiefly in mind when I spoke of after study. I frankly own that for myself when I come across a page of equations, like these, in a text-book I generally skip them. (Laughter.) I find that usually there is enough explanation of the mathematics either before or after them to give me an idea of what is set before me. In the present instance, however, the only symbol which I was perfectly familiar with, while Mr. Reid was reading, was one which he called T square. (Laughter.) I have some practical acquaintance with that. But seriously, I doubt the propriety of bringing extremely mathematical papers before an institution and reading them, unless, as Mr. Kennelly has suggested, we first have them printed and put into our hands so we can digest them before we swallow them, so to speak. For mathematics, however valuable they are (and none can doubt their value, if properly used,) are too frequently like a very well fitted and extremely costly machine shop filled with the highest order of machine tools, owned and operated by some person who with all the necessary skill to work those tools, employs his skill, and employs the time of the machines in doing nothing else but making more machines of a still more costly and

intricate nature. (Laughter.) And thus it appeares to me that we should handle mathematics with a great deal of delicacy, care and judgment, and use them in a mixed multitude only when we have to, the highest function of a technical paper being to instruct.

Referring for one moment to alternating machines *per se*, although it is a little perhaps out of the exact track of the papers before us, and the discussions which should properly follow them. I may say that there is quite a prevalent impression in the land, and quite largely, among gentlemen who suppose themselves to be electricians, that alternating currents used in electric lighting date back only to the time when Gaulard and Gibbs employed them to excite the primary of an induction coil or transformer for incandescent lighting. Within the last two weeks I have heard it gravely asserted by such a gentleman, that the first time an alternating machine was ever used in lighting was on that occasion. It was of very little use to tell him, (for he would not have believed me if I told him and swore to it on a stack of bibles,) that the first machine ever used for lighting was an alternating machine, and the first currents used were for arc lights without commutation. Nevertheless it is true that the Von Malderen or Alliance machine was an alternating machine and achieved better success in arc lighting than the machine subsequently made by Holmes in England in which the currents were commutated. I would not, of course, venture to compare the highly organized machine of to-day, such as the one of which we have heard this evening, with the very crude alternating current machines of past time, for they were simply the magnetic machine of Clark and Saxton multiplied indefinitely; that is, a number of the electro-magnets placed upon a frame were whirled around, the entire effect being due to the multiplication. Whereas in the machine of to-day we have principles which have been learned by experience, and greatest and most valuable of all we have the principle of the magnetic circuit. Nevertheless I have thought it not out of place to call the attention of the Institute to the fact that alternating machines *per se* were old; that alternating currents in electric lighting are not new, and that advance has been made chiefly on the lines of improving and doing on well defined principles, things which have been inferiorly done before our time.

I might mention in conclusion that the Jablochkoff candle was in its time operated also by alternating currents for the reason that if it had not been, the positive carbon would have burned much faster than the other. Therefore, as many of you will remember, instead of using the Gramme machine with its continuous current, the Gramme machine was used to separately excite an alternator, which in turn was used to furnish current for the candles.

Mr. E. T. Birdsall:—There is one point in the paper of Messrs. Tobey and Walbridge about which I would like to ask a

question, and that is in regard to the efficiency. They say that the efficiency varies between 93 and 94 per cent. Now the question I would like to ask is, how the efficiency was obtained from the curves. The reader said that it did not matter whether the volts were taken by the mean square process or by the watt curve—you got watts all the same. I would also like to ask whether the efficiency takes into account the power used by the exciter?

MR. TOBEY :—In reply to the gentleman, speaking of the two values of E. M. F. and efficiency, the former was obtained first, from the square root of the mean square; second, by the taking the watt curve, the points of which were plotted from values obtained from the instantaneous values of current and E. M. F. These gave the instantaneous values of watts at periods; then integrating the curve we obtained the output in watts. In this manner we obtained the value of the watts without determining either the E. M. F. or current. Dividing the watts by the current gave us the second value of E. M. F. and current, but we separated them only as a matter of interest.

The values of efficiency include the excitation. The method of getting these efficiences was to divide the watts electrical output by the sum of the watts by dynamometer, plus watts excitation.

MR. CHARLES STEINMETZ :—In looking through the equations given here by Mr. Reid I see the magnetism of the field, called N, is supposed to be constant. Therefore, the magneto-motive-force of the field magnetism, that is the exciting current is supposed to be constant, too. But that will hardly ever be so, because of the induction, which the varying magnetism produces in the field circuit.

Now here in the diagram given by Mr. Tobey and Mr. Walbridge I see the instantaneous values of the exciting current varying by more than 50%. Therefore it would follow that the field magnetism is varying in the same manner in that kind of alternating generator, and I could not think the equations to be complete, if the field magnetism is supposed to be constant. To complete the equations, there ought to be added to the term N, another factor, *f. i.* of the form;

$$1 - \varepsilon \sin^2 (\theta - \omega),$$

where εN is the amplitude of the variations of exciting current, ω its angle of lag.

Furthermore here I see the magnetism A is supposed to be proportional to the magneto-motive force over the magnetic resistance. And this magnetic resistance is supposed to be constant. I think it would make a certain difference in these equations if, also, this assumption of the constancy of magnetic resistance should be abandoned, as not being quite true. And therefore I think, also, here another term ought to be added; but if this should be done, these equations become so complicated indeed, that I hardly think it possible to integrate them. (Laughter.)

I saw the importance of this reasoning some time ago. I had to test a certain kind of alternator of about the same type as the new Thomson machine, a short time before this was brought out. In this machine the induction was produced by changes of the magnetic resistance, and neither the armature coils nor the field coils were movable. By a calculation similar to the equations here given, where I supposed the exciting current to be constant, and therefore the magneto-motive force of the field constant, too, I found an electromotive force of I think 500 or 600 volts. But after I experimented with the machine, I found the very high voltage of 22 volts! (Laughter.) In this set of tests I used as an exciter the current derived from a storage battery of about 30 volts. Now I tried to find out the reason of this very curious phenomenon, and, suspecting the reaction upon the field current, I used an exciting current which was more likely to be constant, the current of a constant current dynamo, connected in series with a large storage battery and two transformers with open secondaries, as choking coils, so that I was sure that very heavy self-induction and polarization straightened the wave of the exciting current. I got for exactly the same number of exciting turns and the same strength of current, and the same number of periods, the voltage of the dynamo up to 160 volts, still much lower indeed than calculated. So that this consideration seems to be rather important.

Mr. Thorburn Reid:—In regard to the first point, the variation of the field current, I will say that I had gotten the equation (16) before it occurred to me that the field current would be varied; then, when I did think of it, I did not suppose it would be enough to amount to anything in changing the total magnetization. I thought it would be very slight, so I paid no more attention to it.

In regard to the change in the value of ρ, I saw that difficulty from the first. The saturation of course will change the value of ρ all the time. It will be changed all the way from the zero value of the current up to its maximum value. But we have no equation to express this variation and we could not integrate it without such an equation. We have some complicated empirical equations given by different people, which more or less accurately represent the curves of hysteresis, but we have no theoretical equation and none that represents them very closely, so I did not attempt to get an equation which would take in the variation of ρ. I concluded that the best plan would be to get an average value of ρ in different positions of the armature and for different values of the current. That was the only way I could see out of it. But as a theoretical matter I fail to see how these two considerations would materially affect it. Regarding this variation of ρ in the transformer, where ρ varies all the way from the zero value of the current up to its maximum value, if you assume a value of ρ which would be obtained with a constant current of

the same value as the square root of the mean square of the actual current, you will find that the lag will come out much greater than it actually is in the transformer, so that we have to allow for the change in the magnetic resistance due to saturation. This can be done by means of a curve of hysteresis, plotting out the current as a *sin* curve and obtaining the corresponding magnetization from the hysteresis curve, taken from Ewing. I worked that out and found that the lag would be much greater than with the assumed value of ρ I mentioned a moment ago.

MR. C. O. MAILLOUX:—In Messrs. Tobey and Walbridge's paper the curve 4 which is given there to show the polarization of the current, illustrates the phenomenon which occurs in all machines, one might say, though it may not be suspected. It is a fact that in all of the so-called continuous current machines as at present made there is a similar fluctuation. The reactions of the magnetic field are constantly causing a succession of fluctuations, superimposing waves on the current circulating through the magnetic field. If it is a shunt machine, that effect is only felt on the shunt circuit. If it is a series machine it reacts over the whole circuit. In 1886 I had the honor of reading before this Institute a paper on this subject based on experiments which I made at the Philadelphia Exposition. Owing to ill health the paper has never been prepared for publication in the Transactions, but those here who were then present will remember that I called attention in my paper to a method of appreciating these fluctuations and of taking advantage of them so as to detect the extraneous magnetism or what we now call the magnetic leakage. These fluctuations are due to the fact that the armature of any continuous current machine is a series of units which are not infinitely small. If we could make the number of units, or in other words the number of commutator sections, or the number of sections of the armature, infinitely great, then the portions of current commuted would be so small that they would produce no appreciable effect upon the distribution of the magnetic field, and we would not feel their influence. The truth is that each time a section is commuted, or, in an alternating current, each time there is a reversal, there is no re-distribution of the magnetic field. Just previous to the time when the section is about to go out of circuit or is going to have its current reversed, there is a certain tension in the magnetism at or about the neutral line or at the space corresponding to the portion occupied by that coil that is ready to pass out; and at the very instant that the current is reversed in that section, there is a re-distribution. We have a certain relapse, so to speak, of the lines of force. Now this necessarily affects magnetic resistance, since the path of the lines of force is altered in length; and in my endeavors to analyze the phenomena I always regarded it as an increment or decrement of magnetic resistance. If we consider it as such, then it follows that a given magneto-motive force acting on a magnetic circuit

must produce fluctuation when the course of the lines of force is changed, just the same as a change in ohmic resistance in an ordinary circuit would produce fluctuations in the current. Hence, if we take a small coil of wire forming by flexible conductors a closed circuit with a telephone, and place this coil near the field of a dynamo electric machine in a suitable relation to the field, the oscillation of the lines of force, due to this change of magnetic distribution each time there is a commutation, causes the induction of a minute alternating current in this coil whose number of periods or frequency is equal to the number of commutations per second, and causing the telephone to emit a note of corresponding pitch. The paper which I read at the time, showed further how one could trace the course of the lines of force leaking from the magnetic field of the machine in this way by means of the telephone, because if the oscillation is in the same plane as the exploring coil there will be no noise in the telephone and if it is at right angles to the plane of the exploring coil the noise will be loudest.

I have also had occasion to study that fluctuation in a more perceptible way, on a larger scale, in connection with the older form of machines formerly in use, old shuttle wound armatures of the Siemens type. When they are used for producing continuous currents, we get, strictly speaking, not a continuous current, but a series of alternated impulses. We have really a large impulse in one direction followed by a smaller impulse in the opposite direction, in each case, so that the current is only relatively continuous in spite of the commutation. Now I have seen machines of that type where by putting another coil around the magnetic field an alternating current could be obtained which could be used for producing electric light, working transformers or anything of that kind, showing that in this case the oscillations or the wavy action taking place in consequence of the reactions of the armature on the magnetic field, were sufficient to determine a motion of a magnitude adequate to produce the induction of a current in a suitable circuit.

I join with the other gentlemen in expressing regret that we could not get sooner the substance of the able paper read by Mr. Thorburn Reid, in order that we might have the opportunity to digest it and come here better prepared to discuss it. I must say that I, myself, while I could not follow it entirely, have been able to find in it a great many points which are, to say the least, very novel in their method of treatment. I do not deprecate in the least the presentation of papers of that character before this body, because, while I grant that they have a great deal of theoretical value, I feel that they have even more practical value than many of us imagine or are willing to concede. The very circumstance, for instance, that we are enabled to get a clearer perception of the phenomenon of inductance is one which will contribute greatly to the progress we look for in the uses and applica-

tions of the alternating current. I think that the day is coming when we shall be able to express the phenomenon of inductance which is to-day a perplexing and confusing one even to the mathematician, with as little effort and to understand it with as much ease as is now the case with the phenomena and the relations of non-inductive resistances. And in this connection I wish to invite those who study these matters from the mathematical standpoint to consider in connection with their treatment the fact that the elasticity of the lines of force themselves, is a reactionary factor of importance in the phenomena taking place while currents are being induced by the cutting of lines of force.

In expressing inductance we have to introduce the notion of velocity since the E. M. F due to self-induction varies with the rate at which lines of magnetic force are cut. Now the notion of velocity must involve the consideration of the fact that the lines of force themselves are not passive during the operation of being cut; that they may yield one way; that there is a certain tension produced by the reaction of the current which would make them give way, as Mr. Stanley has so well said. Now in that case the velocity would evidently be affected and the rate of cutting would be reduced; while in the rebound they would fall back, and thereby increase the relative velocity or rate of cutting.

DR. M. I. PUPIN:—There is an interesting point which I would like to discuss in a few words and that is, the peculiar form of the curve which expresses the electromotive force. When arc lamps are introduced in the circuit it is evident that the form of the curve is entirely different from the form which we obtain when there are incandescent lamps in it. Mr. Tobey suggests that it is probably due to the change of resistance brought about by the cold air which is cooling off the carbon points, and the experiment which he devised to test this, seemed to prove his suggestion. But there is another way of looking on this matter, and that is this: we know that the current is conveyed between the carbon points by means of luminous carbon particles. The resistance between the carbon points depends not only on the contact between the two carbon points, but also on the amount of the luminous carbon particles which pass between the points. Now, the amount of these luminous carbon particles depends on the strength of the current, just as in electrolysis, the amount of an element deposited by the electric current. Therefore, the resistance will change with the current. When the current is zero the resistance will have a maximum. When the current is a maximum the resistance will be a minimum. Most of the resistance in the arc circuit is due to the resistance between the carbon points. These changes in the resistance of the whole circuit may therefore, be very considerable. In addition to that, we have, of course, changes due to the variation in the temperature of the carbon point. Now, Mr. Tobey says that they introduced the

flame of an alcohol lamp between two points and observed that the sharp corners in the curve were modified—rounded off. But the flame is, comparatively speaking, a very good conductor of electricity, so that by putting a flame between the two carbon points, in order to prevent, as he intended, cold air from rushing in, he has introduced a good conductor there, and has prevented those large variations in resistance, and has thereby prevented sudden changes in the electromotive force.

There is another point which I would like to mention. A gentleman said to-night that it is very desirable to introduce magnetic resistance in the definition of self-induction, for the simple reason that in that case we would have something tangible, whereas the definition of self-induction as given by Neumann is a mathematical one and intangible. Well, I do not agree with that gentleman, for the simple reason that Neumann's integral for the co-efficient of self-induction represents a certain physical quantity which can be measured. It refers to the electro-magnetic energy of the field, and energy is the best conception to have. I do not know what the magnetic resistance is. I cannot imagine anything that would represent magnetic resistance, unless I establish first the relation between this quantity and the electro-magnetic energy. The integral of Neumann represents a certain amount of energy which we can measure in a great many ways. One of those ways, is simply to surround the conductor through which the lines of force run by means of a wire loop whose ends lead to a ballistic galvanometer, break the circuit, destroy that magnetic energy and have it appear as some other form of energy. It will appear in the surrounding wire as a current; the energy which this current represents is the physical meaning of self-induction, or co-efficient of self-induction, if you please. So that the integral, after all, is the best mathematical and practical definition of self-induction; and besides defining the quantity it also suggests to us a method of determining its numerical value.

The third point and the last one which I wish to mention is this: Mr. Reid assumed at first the magnetic resistance of the machine as constant, and assuming that, his equations are all perfectly correct until he gets to equation sixteen or seventeen in which he discusses the resistance ρ. Dr. Steinmetz remarked that if we introduced ρ as a variable we would obtain a differential equation which is not integrable. I do not agree with him in this particular case, for the simple reason that ρ will be a quantity which varies in a certain way and has two points of discontinuation during one revolution. It is a periodic function. It has the same period as the machine. In the second place the function has two points of discontinuity. We can represent every function of that sort by a series of sines and co-sines. But if we look at these curves here which Mr. Tobey obtained we see that these curves are practically sine curves; therefore, we have every reason to infer that the magnetic resistance will be a simple har-

monic function and that this is equal to a constant plus a sine function. Of course if we should be unable to assume that ρ is a simple harmonic function then I confess we would obtain an equation which would be very difficult to integrate. Not so in our case.

DR. CARY T. HUTCHINSON: I do not see how Mr. Reid's equations differ from the ordinary well-known forms based on the assumption of a sine curve variation and constant magnetic permeability.

Another point—in his paper he has given several diagrams of magnetic circuits and based certain explanations upon these diagrams. In Figs. 3 and 4, the lines of induction pass through the armature and back entirely by air. Where is the return part of the circuit? It is certainly not completed through the air as shown. Referring to the definition of the co-efficient of self-induction, is not one of the simplest definitions, the number of lines of induction enclosed by the circuit? This of course includes the case both of air and iron, and takes into account very plainly the reluctance.

Again, I do not see how the reluctance of the circuit can be a simple sine function for the reason that the air gap forms the largest part and is constant.

DR. PUPIN:—I did not say that it is the same sine function. I said magnetic resistance is a simple harmonic function. Of course, magnetic resistance is connected with self-induction. But it does not follow at all that the co-efficient of self-induction will be a simple sine function.

MR. STEINMETZ:—I concede that I did not yet try to integrate those equations. I only doubted their integrability because I several times tried to integrate such complicated expressions by explicit functions indeed, I mean, and I generally got left.

With regard to the magnetic resistance, I believe this term has come into general use, because it gives a very great advantage in numerical calculation, where absolute exactness is not needed, and you are allowed to suppose the magnetic resistance to be constant. But so soon as you enter deeper into the proceedings and try to take into consideration the variability of the term "magnetic resistance," then indeed the matter becomes very complicated, because you can hardly do anything else than define the term "magnetic resistance" as the quotient of magneto-motive force over magnetic current, or number of lines of force, and then the magnetic resistance becomes a very complicated function, which sometimes becomes infinite, too, because sometimes the magneto-motive force may become zero, when the magnetism is not zero, and the magnetic resistance for the same magneto-motive force can have all values from a certain minimum up to infinity, even can become zero. Therefore, in this case, you can not use the term "magnetic resistance" with any advantage, and we have to go back to the old and well-proved manner of mathematical calculation.

MR. TOBEY:—The reason we used the galvanometer at the time we made these experiments was, that there was no suitable electrometer except one which was in use. We afterwards found that, had we used the electrometer we would not have been able to indicate the sudden changes of E. M. F. across the zero line, the throw of the electrometer always being in the same direction; consequently the very sudden changes from a positive to a negative value would not be definite.

DR. HUTCHINSON:—Dr. Duncan of Johns Hopkins University and two associates made some experiments of this kind nearly three years ago; these curves before me might almost be taken as tracings from some of theirs and the description of the method and apparatus used in obtaining them applies almost verbally to their method and apparatus. This is the more remarkable as neither in this paper nor in Prof. Ryan's read before the Institute some time ago and which was even closer to Dr. Duncan's than this one is—as both dealt with transformers—is there the slightest reference to their work. Their experiments were made on a Siemens dynamo using both open and closed circuit transformers. Nor did they find any difficulty in the direct use of a Thomson quadrant electrometer.

MR. REID:—I would like to add a little to the discussion of magnetic resistance by referring to some investigations I made a little while ago on a transformer. I calculated the lag of the transformer, taking the magnetic resistance as that, which we would have with the square root of the mean square of the current and found it entirely too great. Then taking the hysteresis as shown by Ewing's curves and calculating the work lost by hysteresis in the transformer, I found that the lag came out very nearly what was actually found in the transformer, so I had to assume that Ewing's curves were applicable to the transformer. Now if we plot our curves of magnetization by means of Ewing's curves, assuming that the current follows the *sin* law, we will get a curve of magnetization not at all like a *sin* curve. The side where the curve rises is almost vertical and it comes down again to the zero line much more gradually. The curve is almost a rectangle with rounded corners. If that is the case I do not see how we can get the co-efficient of self-induction as a *sin* function. The first investigation seems to show that hysteresis is present in the transformer and the second that the current cannot be a *sin* function if this is so. And yet experimentally the current appears to follow the *sin* law which would appear to show that the co-efficient of self-induction was a *sin* function.

THE PRESIDENT:—The time for our adjournment is considerably past. I do not like to stop a discussion so interesting as this is on so interesting a paper.

MR. TOBEY:—Before adjourning I would like to ask if some members here cannot throw some light on the subject which I brought forward, when I said it was a question in our minds as

to whether or not the square root of the mean square of a rectangular curve, similar to our electromotive force curves, gave the actual E. M. F. We do not understand it, and would like to be enlightened by any one who has had experience in dealing with this form of curve. If the square root of the mean square does not give the actual electromotive force, how can it be obtained directly from the curve?

DR. HUTCHINSON:—Replying to the question as to the cause of the difference between the value calculated from the square root of the mean square and the observed values from the curve, the explanation is simply that the values calculated are based on the assumption of a sine curve, whereas the curves given are very different.

MR. T. C. MARTIN:—Before the notice of adjournment I would like to say on behalf of the committee on papers and meetings that, as Mr. Pope has said, it is our constant endeavor to get the papers here on time, and we have tried and tried in vain to have the papers brought to us sufficiently in advance to have them printed. I think I may say that we have now practically abandoned any serious effort in that direction. I would like to add that this is not the last opportunity at which any paper presented can be discussed. This is not a meeting for the conversion of the unrepentant. We can discuss these papers a month hence and any gentlemen has the privilege of rising on the floor and asking for an explanation or taking any paper which has been presented and offering any remarks on it. I would like to say also in regard to that matter that if we print the papers twice we put the Institute to a great deal of additional expense.

My business in rising Mr. Chairman, was not so much to make that explanation—it is by no means an apology, simply a statement—my object in rising is to present to your notice, and I hope your favorable consideration, a resolution which has been placed in our hands by our friend, Mr. Allen R. Foote, the Secretary of the National Electric Light Association and the special census agent for obtaining electrical statistics for the eleventh census. The resolution is as follows:—

"WHEREAS, Senator Hale, Chairman of the Senate Committee on Census, has introduced a Bill of which the following is a copy:—

A BILL.

TO AMEND AN ACT ENTITLED "AN ACT TO PROVIDE FOR TAKING THE ELEVENTH AND SUBSEQUENT CENSUS," APPROVED MARCH FIRST, EIGHTEEN HUNDRED AND EIGHTY-NINE.

BE IT ENACTED BY THE SENATE AND THE HOUSE OF REPRESENTIVES OF THE UNITED STATES OF AMERICA IN CONGRESS ASSEMBLED, That for the purpose of rendering the investigation of the electrical industries by the eleventh census thorough and complete in every detail, the Superintendent of the Census, under the direction of the Secretary of the Interior, is hereby authorized and directed to make such sub-divisions of the subject as may seem to him advisable, and to associate with the special agent now commissioned to investigate the manufacture of electrical apparatus and supplies and their uses, as special

expert the ablest person whose services can be secured to write the statement for each subdivision so made, subject to the supervision of said special agent; and the sum of fifty thousand dollars, or so much thereof as may be necessary, is hereby appropriated out of any money in the Treasury not otherwise appropriated.

"*Whereas*, it is the opinion of THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS that the enactment of this bill is not only desirable but of the highest importance to the industries and people of this country generally, therefore be it

"*Resolved*, that the proper officers of the INSTITUTE be authorized and directed to make such statements and representations to the Chairman of the Committee on Census of the United States Senate and House of Representatives, as in their judgment are necessary and pertinent, to assist in securing the action desired."

I might say that the National Electric Light Association has already taken action supporting this bill, which has been read twice and referred to the Committee on the Census. A similar resolution will be presented, and I have no doubt adopted by a large number of the electrical bodies throughout the country, and I think personally that it is the duty of the American Institute of Electrical Engineers to give its hearty support to such an investigation and report. I do not think that the electrical industries have the slighest objection to standing up and being counted. At the present time there is the utmost vagueness in regard to the electric interests of the country. In fact, if we take the oldest—the telegraph—there are no statistics to-day of the mileage of telegraph lines in this country except the Western Union. We want that information. A great deal of the work in the future absolutely depends upon it. Then I think that we would be perfectly satisfied with even a Porter census, especially with regard to the people that we are alleged to have killed. We want this information not only as persons who are practically engaged in electrical industries, but also as experts and electrical engineers, and I should be very glad indeed if this resolution meets the support of the meeting.

DR. SCHUYLER S. WHEELER:—I move that Mr. Martin's resolution be adopted.

(The motion was seconded.)

MR. R. W. POPE:—It does not appear to me that we ought to vote money out of our treasury—each of us being interested in the funds of the Government—without further consideration. I am sure this matter is a surprise to me. If we had a committee on the census we might refer it to them. It appears to me that we ought to give this matter careful attention. It is brought up at a late hour, and for my part I do not feel altogether like recommending an appropriation of this kind without thinking it over. This matter has been brought before this meeting to be sure, but we have had no notice of it and I do not think it has had a fair discussion. I would move that it be laid over until the next meeting for action

MR. MARTIN:—I would like to say Mr. President, that there is certainly—although our friend Pope may be too busy to get around to it—there is certainly no lack of information on the subject. I have in my hand copies of a pamphlet of which several thousand have been distributed throughout the country, which was prepared under the authority of the National Electric Light Association and which gives a large amount of information as to what is actually contemplated. It even classifies the various subjects that it is proposed shall be taken up. A great many of those subjects will fall within our own province. I venture to hope that some of our own members will be specially selected as the gentlemen who shall prepare those subordinate and subsidiary reports which shall be placed in the hands of the special agent. The document in question was printed very largely in every one of the electrical papers in this country. I think Mr. Pope himself printed part of it.

MR. R. W. POPE:—Not as coming before this body for action.

MR. MARTIN: Not as coming before this body for action, no. This is the first time the resolution has been introduced. This is a matter which requires time and thought and attention. I thought it had received that. Congress does not meet until December and we can certainly take it up in November. In the meantime we shall see other bodies passing this resolution and I myself, as a member of the Institute, wish to see it take an active interest in the forefront in such a matter.

MR. GEO. M. PHELPS:—I want to say that I do not see any particular object to be gained by laying this over. On the face of it, all can understand it in two minutes. There is nothing to investigate about it. It is very plain and direct. We join with many others who have also been petitioning through members of Congress and otherwise to have that appropriation made. The object of that appropriation certainly cannot fail to have the interest of all members of this association. Electrical people certainly ought to be the very ones to put their shoulder to the wheel and get that thing carried through if possible. I do not think we shall be any better prepared in November to vote on this resolution than we are to-day, and the sooner, therefore we act upon it the better.

DR. W. E. GEYER:—I do not see what special right electricians have to ask Congress to make a special census for them. Manufacturers of all kinds might ask to have special censuses made in their departments and there would be no end of censuses asked for. Then it does not seem to me a dignified position for the Institute to petition Congress for this money especially when it is suggested that some of us might be the ones to get it.

THE PRESIDENT:—I did not understand that the motion to postpone the consideration of this question was seconded.

A MEMBER:—It was.

THE PRESIDENT:—Then the motion is to postpone the consideration of this resolution until the next meeting.

The motion was lost.

THE PRESIDENT:—Now the question is on the original motion. I believe you understand what the motion is without my stating it.

MR. MARTIN:—I may say that our proposal has already had the approval of Congress to a certain extent, that body having listened to the bill twice. There is no intention to put any heavier duties on Mr. Pope than he now has. I can quite sympathize with his anticipations and apprehensions.

The resolutions were adopted and the meeting adjourned.

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